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INDIVIDUAL DIFFERENCES IN WORK CURVES

BY
ELI S. MARKS, Ph.D.

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Error Slip for No. 186: Marks.

Page 40:

Sixth line from top, change W to w^2 (small w).

Fourth line from bottom, *Irregularity* column, change .308 to 3.08.

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Individual Differences In Work Curves

INTRODUCTION

The recent work of Allport and Vernon (1) has drawn the attention of psychologists to a field which till now has been badly neglected in America. The fact that a person's movements may be valid reflections of his personality has long been vaguely recognized, but, with the possible exception of studies of handwriting, American psychologists have devoted little time to experimentation in this field. With respect to the work curve, Mosso long ago observed that "the ergograph thus gives us a record of one of the most intimate and most characteristic features of our individuality—the manner in which we fatigue" (17, p. 92). Most studies using the ergograph have, however, concentrated on the more physiological aspects of work, and little progress has been made in developing the implications of the study of the work curve for personality research.

Throughout this study emphasis has been placed on the psychological aspects of the work decrement rather than on the physiological ones. The writer is in complete accord with the following statement of Robinson (21, p. 580):

> "The ergographic method suffers when it is thought of as an attempt to study in the living individual the type of phenomena observable in the recurrent or continuous stimulation of the nerve-muscle preparation. But if the method is considered as an approach to the activity of the intact and living organism, its lack of simplicity may be thought of as a merit rather than as a deficiency. There also remains the essential fact that, even though the pattern of muscular activity in ergographic work is persistently variable, there is far better hope of understanding such variability here than there is in stair-climbing, tapping, mental arithmetic, or grading compositions. In the writer's opinion this is not an argument against the study of those other activities, but it is an important answer to the criticism that the complexity of ergographic work renders it insusceptible to scientific study."

The first clear-cut recognition of the importance of individual differences in the ergographic curve is the report of Mosso. As early as 1888, Mosso noted that the form of the decrement in the curve of voluntary muscular work obtained on the Mosso ergograph is characteristic of the individual. In a summary of these findings (17, p. 90), Mosso states: "The profile of the curve, that is the line which is obtained by uniting the summits of the contractions, forms a curve which varies in different people. I have been unable to find a reason for this fact, and have ended by convincing myself that it is in truth a constant feature which indicates the diversity which every one presents in his way of becoming fatigued.''

Since the work of Mosso several studies have appeared in which his observation of individual consistency has been confirmed. In nearly every case, however, these studies have depended on the impression of the investigator, or on the visual similarity between samples of work curves of the same individual. The extent to which these similarities may be due to the effects of random sampling has not been considered, and no attempt has been made to get a quantitative statement of constancy.

Of various references to constancy in the work curve, the following are of most interest:

Ioteyko (8, 9) (using Mosso's ergograph) fitted third degree parabolas to work curves of various individuals and found that the constants of these curves remained the same for a given individual under the same conditions. Neifeld and Poffenberger (18) on repeating Ioteyko's analysis (using the Mosso ergograph and also a task involving lifting a weight by bending the arm at the elbow) found that the same individual was by no means entirely consistent in his various work records. While their results are at variance with those of Ioteyko, Neifeld and Poffenberger were primarily concerned with another aspect of the work curve which will be discussed below. Their results are not to be construed as indicating any great inconsistency of the individual's work curves.

Wojciechowski (23) reported that, when the subject works spontaneously, his work curve (dynamograph curve) always has the same character. However, no evidence for this statement is presented. Maity and Sinha (15) analyzed 119 Mosso ergograph curves obtained from 10 subjects. They used two measures of the curve: 1) total amount of work done; 2) a series of ''Fatigue Coefficients'' obtained by dividing each curve into groups of 10 pulls and getting $F.C. = \frac{x - y}{x}$, where x is the work done in the first ten pulls and y is the work done in a particular succeeding set of 10 pulls. The Fatigue Coefficient is thus the percent of decrement in work in a given period. Individuals differed widely both in the total amount of work done and in the fatigue coefficients for their curves, but this difference was greater between individuals than between the various curves of a given individual. Korngold (12) studied the problem of constancy for 500 subjects, using the dynamograph and measuring initial force of contraction and duration of contraction (taken as the time from the beginning until the force has been reduced to one-half its initial value). For initial force of contraction he found correlations between the first and second trials of his subjects and between their second and third trials of about .80 to .85. For duration the correlations between the first two trials were about .60, while the correlation between the second and third trials was .80 or over. This last point suggests that practice may increase the constancy of the results obtained. Korngold was concerned in his measurements more with total effort features than with the characteristics of the work curve. Yochelson (25) studied work curves, using a task in which the subject lifted a weight by flexing his arm. He found characteristic curves for each of his five subjects. These curves did not set in until late in the preliminary practice series, but, after they did appear, they were fairly constant from day to day. The study of Yochelson is reported in detail by Robinson (21), who notes: "Experiments utilizing different muscle groups of these same subjects also indicate that the ergograph curve remains fairly constant for the individual. One might almost say that the curve is a personality trait. The insistent character of these individual differences indicates more than the presence of factors other than energy-depletion and fatigue-products; it indicates that those other factors may remain fairly constant for the reactive system of a given individual. Many investigators have admitted with deep regret their inability to eliminate psychological factors which complicate the problem of the decrement for muscular work. They have assumed that such factors, if present, are bound to be so nebulous as to place them beyond the reach of scientific study. Yochelson's results do not minimize the possible complexity of the decrement for muscular work, but they give a hint that the factors underlying it are definite, even though they are also complex."

There thus seems to be considerable evidence for individual consistency of the work curve; most of it, however, of an impressionistic sort. The present study aims to make a more exhaustive and quantitative study of this problem.

In addition to the problem of *individual* consistency, the work curve also has been examined from the viewpoint of *group* consistency. Assuming that a curve can be found which is characteristic of the individual, is there any evidence for the existence of a curve which is characteristic of certain groups of individuals? The most generalized form of this problem is the question whether there is any general form of the work curve. It should be noted that generalized group differences, or even a general work curve, are by no means inconsistent with marked individual differences within the general group form. The group form sets the central tendency and the range of individual variation.

Ioteyko (9) reports "We have been able to demonstrate that the ergograph curve, independently of the type presented by the subject, is a third degree parabola, whose equation is: $\eta = H - at^3 + bt^2 - ct$." Here η is the height of a single pull, H is the height of the initial contraction, and t is the elapsed time; while a, b, and c are constants to be determined from the data. Ioteyko maintained that the sign of b in the above formula is always positive while a and c are negative. Neifeld and Poffenberger (18) found that their results were markedly different from those of Ioteyko. The present study has pursued the line raised by these two investigations and the question is discussed in greater detail in Section III.

Even though it prove impossible to develop a single formula to cover all ergographic curves, there is the possibility that a limited number of "types" of curve may be found, such that each individual may be fitted into one of these types. Several attempts at such a classification have been made.

Mosso (17) distinguished three kinds of curve, convex, concave, and convex with a long period of high production and then a sudden drop. Lehmann (14) distinguished three types: energetic, anergetic, and fatiguable. The energetic type gives a high initial output continued for some time, followed by a decline to a lower level, which may be maintained for a long time. The anergetic type declines to a low level of production almost immediately after the start of work and maintains this level for considerable time. The fatiguable type starts with a high level of output and then slowly or rapidly declines to a point of total fatigue—the total fatigue point occurs much sooner in this group than in the other two. Lehmann found a small group of cases who did so little work that their curves could not be classified, and he found cases of curves with "protruding tops" (*i.e.*, markedly irregular curves) in all of his three categories.

Skawran (22) found that prolongation of the ergograph curve, similar to that which appeared in Lehmann's first two types, could be produced by making the weight less than the individual's strength, so as to permit nearly complete recovery between strokes. He also found that the energetic type was extremely rare, except among criminals. In his group of 178 normal subjects, the energetic type only appeared once, and then only for weights of 3 and 4 pounds, and not for all weights used. Using the Mosso finger ergograph and a 5 lb. weight, Skawran distinguished five principal types: 1) triangular curve; 2) convex; 3) concave; 4) flat rectangular (somewhat similar to Lehmann's anergetic type); 5) high rectangular (resembling Lehmann's energetic type). These terms have reference to the shape of the curves. There were also curves with "protruding tops" (as in Lehmann's study) in 7% of the cases and "formless" curves in 3% of the cases. Skawran found that most of the formless curves belonged to individuals of his delinquent group and in general seemed associated with a very low energy level. He analyzed his curves in terms of Kretschmer's body type system (13). The concave curves were obtained from leptosomes and athletics but not from pyknics. Of the simple triangular curves (which with their near associates made up 30% of the cases) 50% were those of leptosomes, 25% those of athletics, and 12% those of pyknics, the remainder being mixed in body type. The convex curves contained a greater proportion of athletics and pyknics and mixtures of the athletic and pyknic constitution, and the same was true of the flat rectangular curves, which were in general similar to the convex type of curves. The high rectangular curves were given almost entirely by athletics and pyknic athletics.

In both of the classification schemes described above (Lehmann's and Skawran's) transitional types from one sort of curve to another are as common as, if not more common than, the types themselves. While this is of no importance if the types can be established as actually existing modes of response, some consideration of the distribution of the transitional cases around the type mode is necessary. In general the work in setting up types has been subjective and impressionistic, and no consideration has been given to the actual distribution of the characteristics of the ergograph curves. The consideration of this distribution and the inferences which it leads to regarding types of work curves is the subject matter of Section IV.

A very thorough study of motor behavior from the standpoint of Kretschmer's body-type theory was made by Enke (2). Part of his study involved the use of the Mosso finger ergograph using a 1 lb. weight. The subject worked at the pace most agreeable to him for 30 seconds. The number of strokes made, the regularity or irregularity of the tempo, and the regularity or irregularity of the length of stroke were tabulated. Unlike most other investigators in this field, Enke set up a quantitative standard of regularity. He found that all groups tended to maintain a fairly regular pace, but that the pyknics had a slower tempo than the leptosomes and athletics. Leptosomes, from their behavior and comments, seemed to be striving for regularity and uniformity. Once the task had started, the leptosomes showed greater motivation and made a better record. When Enke's subjects were asked to pull in time to a metronome beating once every 1/2 second, leptosomes and athletics proved less able to maintain the imposed rate than pyknics.

Most of the differences which Enke reports are based on obtained differences in average scores of the groups or on differences in the percent of cases manifesting a given kind of behavior. Enke gives no information about the reliability of these obtained differences. The only marked differences obtained in the ergographic section of the experiment are those between the pure pyknic group and the rest of the subjects. Unfortunately, although Enke had 239 subjects, only 16 of these were of the pure pyknic type. In addition, Enke had 49 subjects of the mixed pyknic type, but the results of this group were quite different from the results of the pyknic group and, in general, do not tend to confirm the conclusions drawn about the pure pyknic group. It is possible to test the reliability of the differences of the percentages which Enke gives, and (mostly because of the unreliable nature of the figure obtained from the pyknic group) these differences are largely not significant statistically. It may be argued that all the differences obtained, even though they may be separately unreliable, yet in mass are to be considered significant. There are two difficulties here: 1) while the differences may tend to corroborate each other, they are undoubtedly correlated, so that no conclusions can be drawn as to general leptosome-pyknic differences (as against differences in this particular sample) unless the correlations are known; 2) the apparent consistent trend of the results is largely a matter of the interpretation put on them rather than of the results themselves, for the "consistency" of the results is largely obtained by framing the interpretations so that the results form a consistent picture. The general trend of results cannot, therefore, be offered as an indication of the reliability of the conclusions.

While most of the work on psychomotility and Kretschmer's types is qualitative and impressionistic in nature, so that it is very difficult to determine its value, in general (and also on an impressionistic level) it would seem that the relations obtained up to now between body types and work curve types can be just as well explained in terms of correlation between two continuous covariants of body build (or possibly ''strength'') and ergograph performance. This by no means precludes the possibility that closer examination of the bivariate distribution of these variables may reveal anomalies inexplicable in terms of correlation. From the reported results, however, no conclusions may be drawn.

In addition to the analysis of the ergograph curve as such, the present study aimed at an investigation of individual differences in work from the standpoint of psychomotor consistency in the sense in which it is used by Allport and Vernon (1). Their work was much wider in the scope of the behavior studied than the present study, in which an attempt at a more intensive analysis of a single function is made. Allport and Vernon (p. 20) distinguish two senses in which a movement may be said to be consistent: ''on the one hand, the term suggests that different indicators of movement must vary directly with one another; on the other hand, it suggests that even when indicators do not agree directly with one another, they may still be harmonious in the sense that they both express different aspects of a single complex state. . . . When in our results indicators are shown by correlation to vary directly with one another, they will be said to *correspond*; indicators that do not correlate, and yet are clearly related to one another through the complex medium of personality will be said to be *congruent.*'' In general the authors try to demonstrate congruence by the method of case study. Case study is frequently very valuable but it remains unsatisfying from the point of view of control and generalization. In the discussion of a case study, interpretation becomes very subtly entwined with data, so that even the most conscientious experimenter will report as obvious fact what is only inferred. The halo effect and the effect of selection of material are other factors contributing to the unreliability of case study. In the case of congruence, what is ''harmonious'' becomes a purely subjective matter, and it is quite possible to consider as harmonious anything which the investigator wishes so to consider. This lack of experimental definition is clear from the examples of congruence which Allport and Vernon give. One of these illustrations is the case of the vocational counselor who was aggressive and self-confident but also cautious, precise, and deliberate. These two aspects of his personality appeared, one in his strolling speed and the other in his walking speed. The question arises whether there is not an inconsistency or disharmony between the two aspects of this man's personality, or, assuming that these aspects are ''harmonious'' (however this term is to be defined), where we are to draw the line between harmony and disharmony. It would appear that we are to accept the two tendencies as harmonious by hypothesis, and then use this hypothesis to explain away the seeming inconsistency between the man's walking and strolling speeds. Since there seem to be obvious difficulties in the use of the congruence concept in practical experimental work, its replacement by correspondence relations would be an advantage. One step in this direction would be in the use of techniques which would take into account dependence other than that of linear correlation. Such techniques are available, although some of them are laborious, and these techniques are discussed in Section V.

The present writer believes that congruence is ultimately reducible to a mathematical relationship of psychomotor indicators which is exact and definite, even though it may be tremendously complicated. This is essentially a form of correspondence. It may be that this result is only obtainable by throwing aside considerations as to the ''ultimate psychological significance'' or the ''phenomenological content'' of the relationships obtained. It is however more likely that, if statistical methods are applied with sufficient care and sophistication to make possible their final interpretation and if this interpretation is carefully made, we shall be able to reach conclusions which will not be vitiated by subjectivity, selection, and sampling errors. There is nothing in statistical methods which makes them the exclusive property of the atomist, and, if the more ''modern'' psychologies are willing to make the effort of deducing the mathematical consequences of their systems, it should prove easy to submit these to a test which will be something more than a matter of individual opinion. In general the criticisms which have been levelled against statistical method (or, as it has been called, ''statisticoid jugglery'') are due to (a) the carelessness of those who apply statistical methods, (b) the carelessness of those who do *not* apply statistical methods.

SECTION I

PROBLEM

As Allport and Vernon (1) point out, before we can attempt to determine the *diagnostic significance* for personality of a particular motor expression, we must discover the *reliability* of the expression. Following the lead of Allport and Vernon, we have been concerned in this study primarily with the reliability of work curves and with their status as characteristics of the individual. It is felt that, unless the work curve can be shown to be characteristic, study of its rôle as a reflection of the ''total personality'' is meaningless.

The aims of the present study of the work curve were: 1) to study the consistency of the individual in muscular work and to determine whether the ergograph curve is (as is frequently claimed) characteristic of the individual; 2) to determine whether any general conclusions regarding the factors underlying the work decrement can be drawn from the shape of the ergograph curve; 3) to measure, insofar as this is possible, the several variable features of ergograph curves and to investigate the distribution of those features in a large sample of subjects; 4) to study the relation of ergograph results to the general concepts of psychomotor consistency developed by Allport and Vernon (1).

PROCEDURE

The apparatus used was a Mosso ergograph in which the weight was replaced by a spring having a maximum tension of 16 kilograms. The finger strap was removed and a metal dynamometer handle substituted, so that the subject pulled with his entire hand instead of with a single finger. A steel wire connected the handle to the spring. The mount for the writing pen of the ergograph was attached to the wire and moved with it. A record was taken on a paper strip which was pulled evenly and continuously over a smooth, vertical, wooden writing-surface by two revolving rubber rollers. The work record was accompanied by a time-line made by a pen attached to a small magnet. The circuit of the time-line magnet was made and broken once every five seconds.

A hand pull was used instead of a finger pull to emphasize the psychological factors involved in ergograph work rather than the physiological factors. Mosso and later writers have pointed out that, in performances utilizing the gross musculature of the body, considerable shifting of the load from one muscle group to another occurs, so that fatigue decrement is delayed. The performance of the gross musculature therefore was felt to be more ''voluntary'' in nature than that of the finger muscles, even though Yochelson (25) reports that the same type of curve is given by a subject, no matter what the muscle groups involved are. Yochelson's subjects were, however, highly practiced in ergograph work. It is likely that we are dealing in his cases with a stereotype resulting from practice rather than with a spontaneous expression of personality. The increase in consistency with practice which Yochelson reports is undoubtedly a real one, but this kind of consistency is largely a specific habit. The present study was more concerned with the spontaneous consistency which might appear prior to the stereotyping of responses. It was felt that the muscle groups used might make a real differences in the results, and that a more spontaneous and more ''psychological'' performance would be obtained if we studied the unpracticed performance of work with the whole hand.

Although the weight ergograph is the form of ergograph used in most of the studies to date, the advantages of the spring ergograph were very early recognized, and these advantages seemed to make its use particularly desirable in the present experiment. Woodworth (24) pointed out that the fatigue curve appearing in the Mosso ergograph is not a true index of the subject's fatigue characteristics, for it is well known that a subject who is apparently completely fatigued will be able to continue working, if a lighter weight is substituted for the original one. Woodworth suggests that the ''true'' form of the curve of ergographic work is obtained when a spring is substituted for the weight.

Most studies of the work curve in which any quantitative analysis has been attempted, have taken as their unit of time, some fraction of the time from commencement of work to the point of complete decrement (*e.g.*, Neifeld and Poffenberger, 18). Other workers have used the actual elapsed time as a measuring unit (*e.g.*, Ioteyko, 8, 9). The use of some more or less arbitrary time unit seems necessary, if any analysis at all is to be attempted. The total work period, however, is frequently a very unreliable measure, for some subjects will experience ''blocking'' phenomena or phenomena of temporary total decrement, *i.e.*, for a moment the subject is unable to continue, and (if he is working with a metronome) he will miss a stroke, continuing on the next stroke at about the same level as the preceding stroke. If total decrement is taken as the point at which the subject fails to respond to the metronome beat, a serious error will be introduced in cases showing ''blocking.'' If missed strokes are disregarded in determining the point of total decrement, the length of the work period may be unduly increased.

For the above reasons a spring ergograph was used in the present experiment and actual elapsed time was taken as the unit. The subject worked for a fixed period of time (2 min. 35 sec.) and not to exhaustion. The work was done with the left hand.

It was felt desirable to permit the subject to vary with respect to speed as well as output. In actually performed work, outside the laboratory, people select their own rate of performance, and rate of work is as important an attribute of personality as is the amount of work which can be done at a given instant. Instead of controlling speed, therefore, it was decided to study it as a variable attribute of the work curve. It might have been possible, as Enke (2) did, to tell the subject to work at the rhythm most agreeable to him. In the spring ergograph, however, this would have the effect of giving unequal emphasis to the two factors of effort exerted and rate of work. We obviously could not instruct the subject to put forth as much effort as was agreeable to him, for such an instruction would have been too ambiguous for most subjects. It was therefore decided to instruct the subject to work at his maximum speed and to put forth maximum effort on any given pull. The instructions were to pull downwards from the wrist on the ergograph handle and then to allow the handle to return to the original position. The subject was told to ''pull back as far as you can each time and make as many pulls as you can.'' If the subject seemed uncertain of the directions, these were repeated until he seemed to understand that *both* maximum effort and maximum speed were to be maintained.

Two curves were obtained from each subject. In no case were the two curves obtained on the same day. This was necessary as many subjects found the task a very difficult one and reported, after completing the work, that their arms felt very fatigued.

Each of the work curves obtained was divided into 15 ten-second intervals. The first five seconds of work were discarded. This seemed necessary, since the subjects were unpracticed and their first two or three pulls involved a tentative adjustment to the apparatus. The second work curve usually showed a more rapid adjustment than the first. The extent of the adjustment was slight for both curves, and omitting the first five seconds of work had little effect on the final results. Since a spring ergograph almost never gives complete decrement of performance, each of the 15 ten-second intervals contained some record of work done.

The subjects of the experiment were 164 male students. Most of the subjects were college students, but there were included a few high school students and some students in the graduate, architecture, law, optometry, and business schools of Columbia University. While some differences in work curves would be expected between students and, say, coal miners, it is very unlikely that the various schools represented in this study would show any differences. The subjects may therefore be considered fairly homogeneous with regard to occupation.

All of the subjects were White. Their national origins were diverse, but it is doubtful whether this diversity would produce any differences in ergographic performance. In this connection, the work of Garth and collaborators (5) is of interest. In a series of studies of mental work Garth failed to find any differences attributable to racial make-up in the curves of mental work among Whites, Negroes, Indians (full and mixed-bloods and nomadic and sedentary), Mexicans, and Japanese. The differences found were all attributable to selection or to nutritional factors. If *mental* work failed to show differences in groups as diverse nationally as those studied by Garth, we should expect little heterogeneity in *muscular* work due to national heterogeneity. It is possible, however, that some of the differences to be reported later can be traced to differences in culture, and, obviously, if ''culture'' is taken as synonymous with ''environment'' (as has been the loose usage of many studies) a great part of the differences must be so traceable. The specific reference here, however, is to national and racial differences. There may be cultural differences in work *secondarily associated* with race or nationality (as, for example, the differences, traceable to differences between nomadic and sedentary habits, which Garth found distinguished the work of some Indians from that of Whites). In the case of our subjects, specific study of the individual's background would undoubtedly be a more fruitful approach to an explanation of individual differences than is an investigation of the possible effects of ''national culture patterns.'' If the effect of ''national culture patterns'' were to be studied, more widely divergent groups than those of the present study should be utilized.

Each subject reported to the experimenter his age, weight, and height. The subjects ranged in age from 16 years to 32 years. Their weights were between 120 lbs. and 214 lbs., and their heights were between 64 inches and 75 inches.

SECTION II

INDIVIDUAL CONSISTENCY IN THE WORK CURVE

To determine just how much consistency there is in the work characteristics of the individual, measurements were made of six features of the work curve. The procedure in obtaining the measurements was as follows: Each curve was divided into 15 intervals, each interval representing a period of 10 seconds. The first 5 seconds of work were discarded in every case, for the reasons given in the preceding section. For each interval of each curve the following measures were obtained:

(a) Number of pulls made in the interval.

(b) Distance pulled (in millimeters) on the middle stroke of the interval. The ''middle stroke'' is the pull in the middle of the interval and not the stroke of median length. The middle pull was selected as a random sample of the pulls in the interval. While there is some decrement in work throughout the interval, the middle stroke is a fairly good representative of the sample of strokes in the interval, and it is not subject to the error which might accrue if the first or the last stroke of the interval were selected.

(c) The range of distance pulled on the various strokes of the interval. This is the difference between the longest and shortest pulls in the interval.

(d) The distance from the bottom of the middle stroke to the baseline. The baseline indicates the point at which there is zero tension against the spring of the erogograph, *i.e.,* the resting position.

From these measures of the various *intervals,* the following statistics were calculated for each curve:

(1) Work Rate: the average number of pulls per interval.

(2) Change of Work Rate: the coefficient of x in the regression equation of number of pulls (y) on time (x). The regression equations were determined by the Method of Least Squares.

(3) Contraction Height: the average length of the middle stroke of each interval.

(4) Change in Contraction Height: the coefficient of x in the regression equation of length of the middle stroke on time. The method of calculation was similar to that for change of work rate.

(5) Irregularity or Variability: the average range of the lengths of the strokes of the intervals.

(6) Tension: the average distance from the bottom of the middle stroke to the baseline. This distance is a measure of the failure of the subject to relax after the pull.

These measures will be referred to, for convenience, in terms of the characteristics of work they supposedly indicate, *i.e.*, work rate, change of work rate etc.

Having obtained measures to characterize the work curves, it is possible to determine the correlation between measures of the first and second curves of each individual. This correlation is a measure of his consistency with respect to the given feature of the curve. The italicized figures in the main diagonal of Table I represent the self-correlations of each of the six factors. The other figures in the table are the intercorrelations of the measures. In Table II are the ratios of z (=the inverse hyperbolic tangent of r) over the standard deviation of z. These ratios indicate the reliabilities of the correlations of Table I.*

TABLE I

SELF-CORRELATIONS AND INTERCORRELATIONS OF VARIOUS MEASURES OF THE WORK CURVE

N = 164	*Work Rate*	*Change of Work Rate*	*Contraction Height*	*Change of Contraction Height*	*Irregularity*	*Tension*
Work Rate	*.790*	–.069	–.641	–.115	.605	.507
Change of Work Rate		*.585*	–.032	–.104	–.114	–.160
Contraction Height			*.778*	.204	.394	–.208
Change of Contraction Height				*.555*	–.031	.101
Irregularity					*.670*	.315
Tension						*.617*

* Since the standard error of z equals $1/\sqrt{N-3}$, it is much simpler than that of r. For our correlations with N =164, the standard error of z is 1/12.689. To get the reliability of r therefore, we need only look up the corresponding value of z in Table V. B. of Fisher (4) and multiply by 12.689. If N is constant and the number of correlations to be tested for significance large, this procedure is much more convenient (and more accurate because of the normality of the z distribution) than getting the standard error of r and dividing r by it directly. In this experiment we shall call significant, results whose probability of occurrence is .01 or less. For a normally distributed variate this represents a deviation of 2.5758. If the ratio of a statistic to its standard error is given without any accompanying probability, the statistic is assumed to follow the normal distribution.

TABLE II

Ratios of z's Corresponding to the Correlations over S.D. of z

	Work Rate	*Change of Work Rate*	*Contraction Height*	*Change of Contraction Height*	*Irregularity*	*Tension*
Work Rate	13.70	.89	9.64	1.52	8.88	7.10
Change of Work Rate		8.50	.38	1.27	1.39	2.03
Contraction Height			13.19	2.66	5.33	2.66
Change of Contraction Height				7.99	.38	1.27
Irregularity					10.28	4.19
Tension						9.01

The self-correlations of the various features are moderately high, all of them being significant. It is possible, however, that much of the constancy of an individual's performance may be attributed to his muscular strength alone. If we consider irregularity, we must note that it is a measure obtained by averaging the range of an individual's pulls within a given interval. It is quite likely that range is dependent on the absolute magnitude of the strokes. It is possible that a coefficient of relative variability should have been used instead of one of absolute variability. In their study of personal variability and consistency, Allport and Vernon (1, p. 124) state: "A subject whose simple reaction time, let us say, is 100 thousandths of a second would obviously have much less chance for variation than another with a mean reaction time of 200 thousandths of a second." They suggest that several investigations of variability have been weakened by neglecting to use the relative variability coefficient in place of a measure of absolute variability.

It does not seem to the present writer that a lower reaction time "obviously" means less chance for variation. If the individual is near the lower or upper limit of measurement, it is true that his possibility of variation may be curtailed on one side or the other, but, for individuals drawn from the same distribution and for distributions having an infinite possible range, a lower score is no necessary indication of either higher or lower probability of variation. It is quite possible that a lower score may, in particular tests, be associated with a high or low individual variability (this question will be considered below in some detail), but, within a single homogeneous distribution, to take the probability of variation proportional to the individual's score is an arbitrary and misleading procedure. There is a further objection to the use of the coefficient of variability in the fact that many measures in psychology have no fixed point of reference, so that one zero point in these cases is as good as another. Consequently, it is possible to make a wholly arbitrary change in the relative variability coefficient by changing the zero point. Allport and Vernon encountered this difficulty but apparently drew no general conclusions from it.

A further objection to the use of the coefficient of relative variability is the fact that dividing by the original score may introduce peculiarities into the results which depend on the denominator of the fractions alone. Thus, if variability were actually equal for all individuals, dividing by the original score might give a correlation between two measures of variability which was dependent solely on the correlation of the original scores.

Avoiding the use of the relative variability coefficient, it is yet possible to determine the self-correlation of our measure of irregularity apart from the dependence of that measure on the absolute contraction height. The simplest way to do this is to partial contraction height out of the total self-correlation. This has been done for the other measures also, and the results appear in Table III.

TABLE III

Partial Self-Correlations of the Measures, Eliminating Contraction Height

	Work Rate	*Change of Work Rate*	*Change of Contraction Height*	*Irregularity*	*Tension*
Total Correlation	.790	.585	.555	.670	.617
Partial Correlation	.642	.585	.535	.609	.600
z for Partial r	.76	.67	.60	.71	.69
$z/S.D._z = 12.649\ z$	9.61	8.48	7.59	8.98	8.73

It will be observed that eliminating contraction height has little effect on the correlations. The only substantial effect is on the self-correlation of work rate. There seems to be in each case a sizable constancy in the measures even with the contraction height eliminated. This constancy of performance relates not only to those features of the work curve which the subject might be expected to control voluntarily, *i.e.,* the output measures—work and contraction height—but also to such aspects as irregularity of work and tension which are not measures of output. It would seem then that the work curve can yield reliable information about the individual in addition to information about his mere output capacity.

If we refer to Table I, it will be noticed that (besides the self-correlations) there are three correlations exceding .50. These are the correlations between work rate and (1) contraction height, (2) irregularity, (3) tension. It is significant that these three correlations all involve work rate. If we examine the work curves, the significance of the correlations becomes apparent. The individuals who work at high speed seem less concerned with attaining maximum contraction height, give less regular curves, and fail to return to the baseline after the pull. Those who work at lower speeds tend to give a full regular pull, returning to the baseline after each stroke. The persons working at high speed are under greater pressure, there seems to be an actual ''drive'' to maintain speed, and height of contraction and regularity are sacrificed to this major objective (failure to return to the baseline may be thought of as a kind of irregularity also). The curves of the slowest individuals seem to show more deliberations, more emphasis on attaining maximum contraction height each time. There are of course exceptions to these generalizations, and these exceptions may be interesting in themselves, but the only method of studying the exceptions here is case study, and the disadvantages of this method have been indicated above. The generalizations about the drive for speed vs. deliberateness are also partly based on case study, but here we have a quantitative check in the obtained correlations. The exceptions may actually represent true tendencies of work for the given individual but their number is not sufficient for quantitative analysis. Possible methods for studying these exceptions are, however, discussed in a later section.

As measured by the self-correlations, there seems to be a fair degree of consistency in the characteristics of the work curve. This does not give the entire picture of individual consistency. Computation of a reliability coefficient will indicate the consistency of the group with great precision *if the distribution of the two successive measures of the function is a bivariate normal distribution.* This will in general be the case if the amount of shift between tests is subject to random determination. It may be, however, that some individuals are inherently more consistent than others. It has frequently been suspected that this is the case, and various writers have pointed out that some of their subjects shifted more than others. References have even been made to a general factor of "stability."

It is not enough, however, to demonstrate that some individuals show larger shifts than others, for this phenomenon may be a matter of random variation. Methods for determining the departure of the obtained results from those expected on a basis of random variation are necessary. One line of attack on this problem is the study of the correlations of an individual's variability in one trait with his variability in other traits. Work along this line is reviewed by Allport and Vernon (1) who also analyze their own results from this standpoint. They attempted to distinguish two general factors of variability: "general variability" and "specific variability," one being a tendency to vary in respect to measures having more general nature (Allport and Vernon's three psychomotor group factors), and the other a tendency to vary on specific motor tests. In general Allport and Vernon's variability factors do not seem to be statistically reliable, and their method of locating the variability factors is based on total impression rather than on careful analysis.

The other line of approach to the problem of individual consistency is through the study of change of position in separate tests. Here we may study the distribution of shifts in score from first test to retest, and determine the extent to which a departure from normality is indicated. This has been done for each of our six measures, as follows: the first measure was subtracted from the second measure and the differences tabulated in a frequency distribution; the first four moments of this distribution were computed and the distribution was tested for skewness and kurtosis. The skewness measure used was the ratio of the third moment around the mean to the cube of the standard deviation; the kurtosis measure was the ratio of the fourth moment around the mean to the fourth power of the standard deviation. For $N=164$, the measure of skewness has a standard error of .191 and the measure of kurtosis a standard error of .382. The ratio of these measures to their standard errors is not distributed normally but the error made in assuming normality is not very large for a sample of this size. (For an indication of the amount of error made in assuming normal distribution of the skewness and kurtosis ratios see Fisher–3). The skewness and kurtosis for the various measures appear in Table IV. The expected skewness of the normal distribution is zero, the expected kurtosis 3. The ratio of the excess of skewness and kurtosis (over that of the normal distribution) to its standard error is also given in Table IV (reliability columns).

TABLE IV

Departure from Normality in the Distributions of Differences Between Corresponding Measures of an Individual's Two Work Curves

	Skewness	*Kurtosis*	*Reliability*	
			Excess of Skewness	*Excess of Kurtosis*
Work Rate	.294	5.197	1.54	5.75
Change of Work Rate	.190	4.620	.99	4.24
Contraction Height	.925	5.724	4.84	7.13
Change of Contraction Height	.321	3.312	1.68	.82
Irregularity	.039	3.787	.20	2.06
Tension	.143	4.813	.75	4.75

In general Table IV gives evidence in favor of the hypothesis that some individuals are naturally more variable than others. On the basis of this hypothesis we should expect an unduly large portion of the group giving shifts very near zero and an unduly large proportion giving very large shifts. The total distribution in consequence would be leptokurtic in shape. This is the case for all the variables, although the excessive kurtosis is insignificant in the cases of change of contraction height and of irregularity.

The fact that the excess of skewness is positive in all cases is interesting when taken in conjunction with the change in the average from the first to the second measure. In all cases this change has the same sign as the skewness of the distribution of shifts, (*cf.* Table V). The evidence is, then, that the change in the average does not indicate a general shift of the group but rather a large shift on the part of a few individuals. This finding bears a relation to the findings on heterogeneity of the original distributions, which is discussed in Section IV. It is likely from the obtained skewnesses and kurtoses that there is heterogeneity in the distribution of variability from time to time also. Our general conclusion must be that some individuals are reliably different from others in variability.

TABLE V

DIFFERENCES BETWEEN THE AVERAGE VALUES FOR THE MEASURES OF THE TWO WORK CURVES

	Mean of Curve I	*Mean of Curve II*	*Difference*	*S.D. of Difference*	*Ratio* $d/S.D._d$
Work Rate	13.36	15.19	1.83	.279	6.57
Change of Work Rate	– .064	– .041	.023	.023	1.00
Contraction Height	27.03	28.17	1.14	.469	2.43
Change of Contraction Height	– 1.117	– .863	.254	.045	5.64
Irregularity	6.38	7.18	.80	.176	4.55
Tension	5.68	6.18	.50	.315	1.58

In some cases the differences in variability seems to be related to the original position of the subject. In Table VI appear the correlations between the first measure obtained from each individual and the difference of his first and second measures.

TABLE VI

CORRELATIONS BETWEEN FIRST MEASURE AND THE DIFFERENCE OF THE FIRST AND SECOND MEASURES

N = 164	*Work Rate*	*Change of Work Rate*	*Contraction Height*	*Change of Contraction Height*	*Irregularity*	*Tension*
Correlation	–.003	–.519	–.298	–.429	–.404	–.184
z	.00	.57	.31	.46	.43	.19
$z/S.D._z$	.00	7.23	3.90	5.84	5.46	2.41

Although the correlations for work rate and tension are insignificant, and that for contraction height is rather low, the general trend of the results seems to be in the direction of a regression towards the mean. The correlations are all negative in sign, indicating an association between high scores and large negative shifts (*i.e.*, shifts toward the mean) and between low scores and large positive shifts (also shifts toward the mean). The fact that this tendency does not appear with respect to work rate may be due to the greater constancy of this characteristic. The relationship of variability to the original position does not, however, completely explain non-normality of the distribution of shifts, although it may have some relation to that phenomenon.

While we have reported a fair amount of consistency for speed, contraction height, regularity, etc., nothing has as yet been said about the *shape* of the work curve, a feature which is much emphasized in previous work in this field. In general the main distinction in shape of the work curve seems to be one between concave and convex curves. To study this feature of the curve, a random sample of 50 was drawn from the group of 164 subjects, and third order regression equations were fitted to the heights of the middle stroke of each interval (by the method of orthogonal polynomials—*cf.* Fisher, 4). This gave four coefficients characterizing each curve. The curves are of the form: $y = A + Bw_1 + Cw_2 + Dw_3$, and the coefficients referred to are A, B, C, and D.* We are here primarily concerned with C and D, for A is a measure of average contraction height and B of linear change of contraction height.

The correlation between the values of B, C and D for an individual's first curve and the corresponding values for his second curve are given in Table VII.

TABLE VII

SELF-CORRELATIONS OF THE COEFFICIENTS OF THE REGRESSION CURVE OF CONTRACTION HEIGHT ON TIME

N = 50	*Correlation*	*z*	*z/S.D.$_z$ = 6.856 z*
B	.360	.38	2.47
C	.199	.20	1.37
D	.113	.11	.75

None of these correlations is significant, not even the B coefficient. B is the same as the measure of change of contraction height used above. The self-correlation of change of contraction height of the entire group of 164 subjects is significant. The difference between the self-correlation of B and the self-correlation of change of contraction height is .25 in terms of the z function. The standard error of this difference is .1658 so that the difference is 1.51 times its standard error. The difference in the two correlations is thus not excessive, and is undoubtedly due to random sampling variations.

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* w_2 and w_3 are second degree and third degree polynomials in w_1 which is in terms of elapsed time. If w_3 and w_2 were expressed as polynomials in w_1, the coefficient of w_1^2 would be C and the coefficient of w_1^3 would be D, while the coefficient of w_1 would be decreased (in our case) by 23.4 D. C and D may be taken therefore as coefficients of the square and cube of the time in an equation of the form: $y = a + bt + Ct^2 + Dt^3$. B will be changed slightly in such an equation, but the value of B obtained here is the value which would be gotten if a straight line were fitted and the change in B would be in the direction of making it more negative. As far as reliability is concerned, substituting t and its powers for the orthogonal polynomials w will change only the reported reliability of B in Table VII.

The fact that the self-correlations of the C and D coefficients are so low indicates that shape of the curve is not a particularly reliable phenomenon in our study. The fact that the self-correlations are decreasing in the order BCD suggests that terms of higher order would yield no greater reliability and general inspection of the data tends to confirm this sugestion. Therefore such departure from linearity as exists here cannot be considered characteristic of the individual.

It is possible that, while C and D are individually not characteristic of the individual, concavity or convexity of the curve may still characterize the individual if we take the joint influence of C and D into account. To a certain extent this may be accomplished by considering the signs of C and D only and examining the constancy of the different possible combinations. (It should be noticed that B is practically always negative). There are four possible combinations of the signs of C and D: ++, +−, −+, −−. If a contingency table is constructed classifying the signs of the individual's two curves, the departure from independence may be measured by getting the expected frequency in each cell and calculating Chi-Square. The value found was 10.61 and (since a four-by-four contingency table has 9 degrees of freedom) the probability of obtaining the result is .31. The amount of association between the sign combinations of the two curves of each individual is therefore not sufficient to indicate dependence between the two curves. Analysis of the signs of C and D confirms the conclusion that the shape of the curve is not constant for a given individual. While this conclusion is not in accord with previous findings, it is probably correct enough for the task studied. It is likely that the reported constancy of shape for the weight ergograph actually obtains, and that the present finding represents characteristics specific to the spring ergograph. There exists the possibility, however, that such individual consistency as has been reported for the shape of weight ergograph curves may be a product of sampling, of practice, and of subjectivity in ascertaining constancy.

SUMMARY

Analysis of the curves of work indicates that various features of the curve remain constant for the same individual when he works on a spring ergograph on different days. Constancy was found with respect to work rate, change in work rate, contraction height, change in contraction height, irregularity, and tension. No constancy was found with respect to the shape of the curve. Individuals differ significantly in the degree of consistency they show.

SECTION III

The Generalized Work Curve

In the Introduction the work of Ioteyko (8, 9) on the general form of the curve of muscular work is reviewed. Ioteyko found that the curve for work with the Mosso finger ergograph using weights is a third degree parabola, having the equation $y = a + bx + cx^2 + dx^3$. This curve represents the regression of the height of the individual contraction (y) on time (x). Ioteyko took as the a constant the height of the initial contraction, and the other three constants were then determined from the data. As is pointed out by Neifeld and Poffenberger (18) taking the initial contraction as the a constant is an arbitrary procedure, since the initial contraction is a single value which is markedly subject to errors of random sampling. The accuracy of the fit can be greatly improved if the a coefficient is determined in the same way as the other three constants. Ioteyko believed that the three terms in x above indicated three factors governing the work decrement, the influence of these three factors being proportional respectively to the first, second, and third powers of elapsed time. The b and d coefficients were supposed to be negative, and the c coefficient, positive. The b coefficient was supposed to indicate a decrement due to reduction in the carbohydrate reserves; the c coefficient, an increment due to the action of the nervous centers in opposing fatigue; the d coefficient, a decrement due to local accumulation of fatigue products.

Neifeld and Poffenberger had their subjects pull to exhaustion, lifting a weight by bending the arm at the elbow. They divided the curves obtained into 15 sections and for a given individual averaged different curves of the same performance by averaging each of the fifteen sections of the curves separately. They then fitted third degree parabolas to these composite curves using the method of moments (*cf.* Rietz—20). (In fitting polynomials the method of moments gives the same results as the method of least squares.) For their curves the a coefficient represents not the initial contraction but the general contraction level throughout the curve. x is here the particular interval corresponding to the given y (rather than actual elapsed time), and the b, c, and d coefficients are similar to those of Ioteyko. For nine subjects, Neifeld and Poffenberger found that b was always positive, c was always negative, and d was positive in 8 cases out of 9. Six curves obtained under different conditions from one subject using the Mosso ergograph, showed 5 cases in which the coefficient signs were similar to those for the arm contraction and one case in which the signs agreed with Ioteyko's claims.

Neifeld and Poffenberger agreed with Ioteyko in finding that their curves were rather well fitted by third degree parabolas. Woodworth (24) and others have suggested that the form of the curve for the spring ergograph is markedly different from that for the weight ergograph. Our curves for the spring ergograph using hand contractions were analyzed to see in what way they differed from the results reported for the weight ergograph (Ioteyko and Neifeld and Poffenberger). Third degree regression equations were fitted to the curves of contraction heights obtained from each of 50 subjects and the results of this analysis are given in Table VIII. In fitting the curves the method of orthogonal polynomials was used.

TABLE VIII

MEAN VALUES, VARIABILITY, AND SIGNS OF B, C, AND D IN WORK CURVES ($y = A + Bw_1 + Cw_2 + Dw_3$)* OBTAINED FROM 50 SUBJECTS

	Mean	*S.D.*	*M/S.D.*$_M$	*Number of Cases in Which Signs Were:*		
				Plus	*Zero*	*Minus*
B	–1.137	.536	15.59	1	0	49
C	.0286	.050	4.00	34	7	9
D	.00096	.0529	.27	18	6	26

* See footnote on p. 25.

The values given for B, C, and D in Table VIII are average values for the group of 50 subjects. In dealing with functions showing marked individual differences, averages are to be used with great caution. However, it should be noted that, in the case of the B coefficient, there is great agreement in sign, all except one of the subjects giving negative B coefficients. There would seem to be a general tendency characterizing the group for this coefficient at least. The C and D coefficients show less agreement but there is a significant difference from zero in the case of the C coefficient. The D coefficient is not significant on the average, and it is very probable that it is also not significant in most of the individual cases. In addition it may be noted that the C and D coefficients are not markedly dependent on the individual, for their retest reliabilities are

.199 (C) and .113 (D) (Table VII). The C and D coefficients (insofar as they are significant at all) seem to be distinctive of the particular curve of work rather than of the individual giving the curve. Furthermore, contraction height and the change in contraction height (equivalent to the A and B coefficients of the curve) are both normally distributed (*cf.* Section IV). It was felt that 50 cases was not a sufficiently large sample for the determination of normality in the case of the C and D coefficients, but it is likely that they also are normally distributed. The distributions of the three coefficients may therefore be assumed to be fairly homogeneous and normal.

For these reasons it was felt that taking the mean of the coefficients as representative of the work curve for the group was a valid procedure, at least for purposes of approximate analysis. It is doubtful whether any important individual peculiarities are overlooked by this procedure.

The averages of the B and C coefficients tend, in general, to agree with the results of Ioteyko and not with those of Neifeld and Poffenberger. While the validity of the sign of the C coefficient may be questioned, there seems to be little doubt of the marked tendency for the B coefficient to be negative, while Neifeld and Poffenberger found that b (which roughly corresponds to our B coefficient) was predominantly positive in their experiments. If we consider the relative numbers of positive and negative B coefficients in our experiment, the probability of getting the results obtained by random sampling is about 10^{-13}. The writer believes the explanation of the difference is to be sought in the difference in the task. Before attempting this explanation some attention should be given to the apparent agreement of the results with those of Ioteyko.

If Ioteyko's hypothesis as to the nature of the components producing the work decrement is to prevail, a much greater agreement with the hypothesis is necessary than was obtained either by Neifeld and Poffenberger or in the present study. In Ioteyko's work the curve is viewed not as an empirical curve of best fit, but rather as an actual expression of the form of work and the factors determining work. These factors are physiological in nature, and, if they are to have any validity at all, they should operate in all subjects so as to give similar results. Except for B there seems to be no evidence for any such uniformity. With respect to B the hypothesis of linear decrement due to consumption of carbohydrates must be rejected also. Numerous writers have demonstrated (*cf.* Robinson

—21) that consumption of carbohydrates is a minor factor in fatigue of the intact voluntary musculature. In addition the factor of linearity is deceptive, for length of stroke on the spring ergograph and length of stroke on the weight ergograph mean different thinks in terms of work done.

In spite of a superficial agreement with the results of Ioteyko, our results are to be interpreted, as suggested by Neifeld and Poffenberger, as merely an empirical fit to the obtained data rather than as direct expressions of underlying physiological factors. There is however no question but that the form of the empirically fitted curve, since it is dependent on the form of the obtained curve, is an expression (indirect it is true) of the physiological and psychological factors involved.

In general the difference between the curve obtained by Neifeld and Poffenberger and our curve is a difference between convex and concave curves. In the curves of Neifeld and Poffenberger the decrement does not commence immediately, the initial part of the work curve being a period of increasing or constant output. This seems to be followed by a period of accelerating decrement until complete exhaustion is reached. The last part of the curve may, however, present a short period where work remains constant at a low level. In our curves the reverse sequence seems in general to be the case. Decrement commences immediately after the commencement of work (provided it commences at all), followed by a period of negatively accelerating decrement until a level of constant output is reached.

For the group of 50 subjects the average A coefficient is 23.28. If we take the average B, C, and D coefficients as representative of the group central tendency, we obtain the following composite regression equation:

$$y = 23.28 - 1.137\ w_1 + .0286\ w_2 + .00096\ w_3.$$

While the w's are not time, they are simple functions of the time and therefore the regression equation may be plotted as a function of time (*cf.* Fisher 4). This is done in Figure 1. It should be noted that Figure 1 conforms to our analysis of a concave curve with initial rapid descent and negative acceleration. There is a flattening out at the end of the curve (in fact there is a slight rise which is undoubtedly an artifact of the curve-fitting). The negative acceleration of the curve can also be seen from the second differences which are all positive (going from left to right on the graph).

One possible explanation of the obtained difference between our curves and those of Neifeld and Poffenberger is in the warming-up effect, if it were present. As far as could be determined from direct observation of the curves, warming up was slight and of very short duration. Warming-up was principally a warming-up with respect to speed, which does not enter directly into the curves which we are here considering. Neifeld and Poffenberger also report that the warming-up effect in their curves is slight. For these reasons warming-up must be discarded as a probable explanation of the difference.

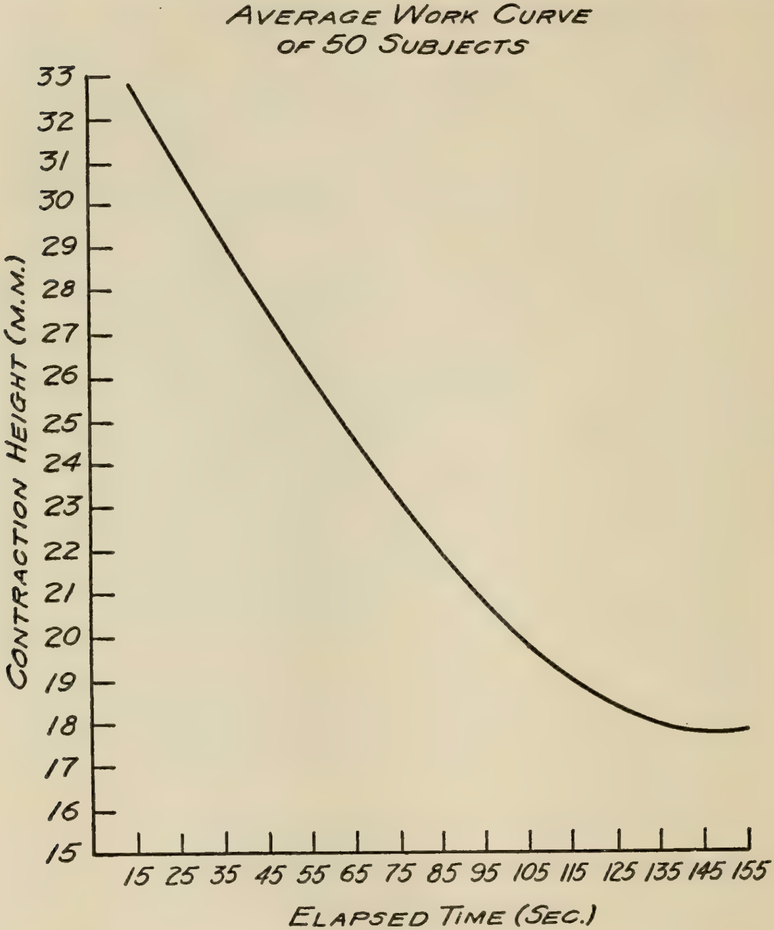

FIGURE 1

The respective forms of the weight and spring ergograph curves obtained, confirm the suggestions of Robinson (21) that, in continuous work, the organism tends to establish a level of output, possibly lower than the initial output level, and that this level is such that it can be maintained for a long time thus protecting the subject against a final complete decrement. Experimenters who have used the weight ergograph, however, have found that a complete decrement does occur. Robinson suggests that this is due to the fact that the subject has less freedom to adjust his performance to an equilibrium level, because a definite standard and rate of performance is set, and any departure from this standard can be easily detected. The ergograph performance therefore differs from performances with less sharply defined standards of output in that there is greater subjective resistance to change of output.

Robinson's explanation accounts for the difference between weight ergograph studies and our own results. In our case the standard of output was more subjective than in the weight ergograph (the subject was told to pull back as far as he could each time), and the rate of work was similarly left more or less to the discretion of the worker (he was told to make as many strokes as he could). For these reasons it was possible for the subject to avoid complete decrement by lowering the standard of performance. In addition, it may be noted that decreases in distance pulled are more effective in lightening the task in the spring ergograph than they are in the weight ergograph, for a decrease in the distance pulled means a decrease in the opposing resistance as well as in the work done.

In some cases the subject set up for himself certain standards of output and speed, and, in consequence, the attempt to maintain these standards produced results similar to those which have been reported for the weight ergograph. There was in particular a strong tendency to maintain constant speed in some subjects. The subjectively imposed rhythm had somewhat the same effect as an externally imposed metronome rhythm might have had. If the adopted rhythm were too fast, almost complete docrement would be expected. This analysis of the attempt to maintain certain subjective standards is based on subjective impression of the characteristics of the curves and it must be taken with considerable caution. The number of cases in which the rhythm was fast enough to cause nearly complete decrement is too small to permit statistical analysis.

It should be noted that the shape of the curve is dependent on the unit of measurement used. Neifeld and Poffenberger give their curves in terms of work done in a given fraction (1/15th) of the total work period. Our curves are plotted with an ordinate which is roughly proportional to the square root of work done (distance pulled against a spring resistance) and with an abscissa proportional to the elapsed time in seconds. While our ordinate is, in terms of physical units, an arbitrary one, there is no reason for considering work done a better unit, psychologically. In studying the work decrement the best unit might be the unit which the subject uses in evaluating his output. In the present experiment the subject was asked to pull back as far as he could each time. This tended to set up a standard of distance pulled as a measure of performance and our work curves are in terms of this standard. The matter is, however, not as simple as this, for, although the directions gave a bias towards distance as a subjective measure of output, the resistance encountered in pulling may have had the effect of setting up another standard in terms of resistance overcome. This standard would be approximately one of work done. In addition to this there may have been a third subjective standard of performance in terms of "effort" or possibly of the "cost of work" to the subject. The cost of work in doing a given unit of work is not necessarily directly proportional to the amount of work done. It is altogether likely that the ratio of the "effort" exerted in pulling the spring the last few millimeters to the "effort" exerted in pulling it the first few millimeters is greater than the corresponding ratio of the amounts of work done, and much greater than the corresponding ratio for the distance pulled.

It is therefore very difficult to determine a "proper" unit for the measurement of work curves. However, if there exists, as is suggested above, a tendency for the curve to reach a level where output becomes constant, this tendency will appear if we measure any one of the units of output discussed in the last paragraph. It will in fact appear if y and x are *independently* subjected to *any* change of unit. The tendency is one for the curve to approach a straight line parallel to the x axis, and a change of unit will not change the slope or the linearity of such a line. Of course we are dealing with curves of best fit and a change of unit will modify the curve of best fit, but the hypothesis here advanced is of an actual tendency to approach constant output and this tendency should appear regardless of the unit of measurement.

SUMMARY

Analysis of the general shape of the curve of work on the spring ergograph indicates a tendency to concave curves in this type of work. This contrasts with a tendency toward convex curves in work on the weight ergograph. The average curve of work on the spring ergograph seems to decrease with negative acceleration to a point beyond which output is constant. The attaining of a constant output level is not present in the weight ergograph curve, where a complete decrement usually occurs. The occurrence of the complete decrement seems to be a function of the greater restriction placed on adjustment by the instructions and the conditions of work. The subject's task in the present experiment allowed him enough latitude to make adjustment possible, and, in consequence, complete decrement was avoided.

SECTION IV

HETEROGENEITY IN WORK CURVES

The last section deals with the question of general tendencies distinguishing the work of the group as a whole. The possibility exists, however, of generalized tendencies, which apply only to certain subgroups within the entire sample of subjects. The presence of such subgroups should introduce a certain amount of heterogeneity into the obtained results, and this heterogeneity may be detected by suitable statistical analysis.

As is reported in Section II, six measures were obtained from each of the curves of the 164 subjects of this experiment. These measures are respectively measures of work rate, change in work rate, contraction height, change in contraction height, irregularity, and tension. In Figures 2 to 7 we see the distribution of the six measures of the 164 cases. In the case of contraction height and change of contraction height (Figures 4 and 5) the distributions are approximately normal in shape. In the distribution of work rate (Figure 2) there seems to be some departure from normality. The distribution of irregularity of the work curve (Figure 6) and the distribution of change of work rate (Figure 3) also show departures from normality. In particular, the distribution of change of work rate shows a strong concentration of cases about the mean, together with an abnormal number of cases in the lower part of the curve. The tension distribution (Figure 7) is markedly skewed, showing a great piling up of cases at the lower part of the curve.

The skewness of the tension distribution is readily explainable as due to the curtailing of the distribution by the conditions of the experiment. The tension measure is obtained by averaging the distance from the bottoms of the middle strokes to the baseline. In most cases individuals came back to the baseline after each stroke. It was impossible for them to go beyond this point because of the construction of the apparatus. (The carriage holding the writing pen came up against a block when tension on the spring was released). Negative scores were thus impossible and the result was a piling up of scores at zero.

In the case of work rate, change in work rate, and irregularity we seem to have non-normality due to heterogeneity, for curtailing of the ends of the distribution does not obtain. There are, in addi-

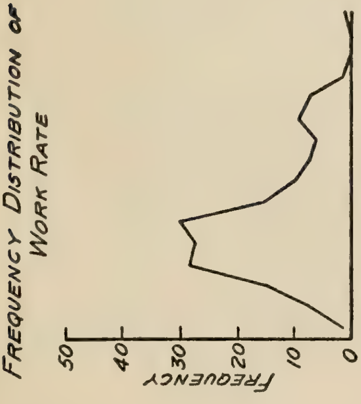

FIGURE 2

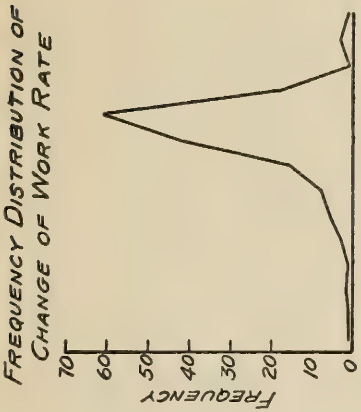

FIGURE 3

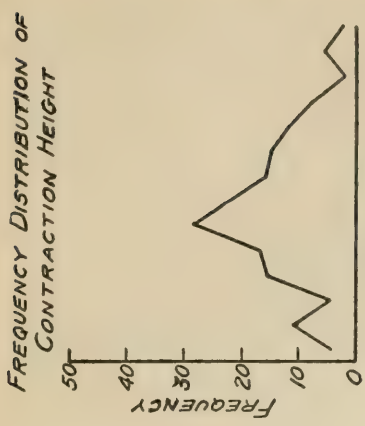

FIGURE 4

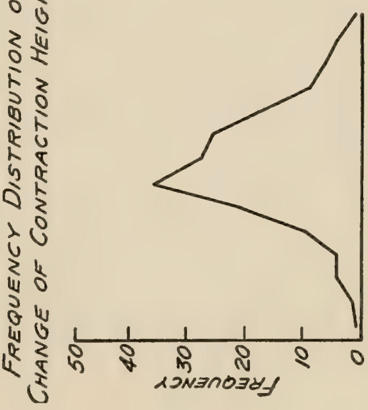

FIGURE 5

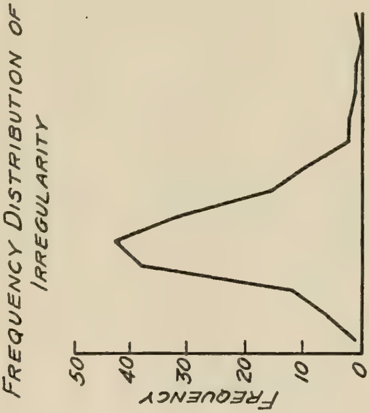

FIGURE 6

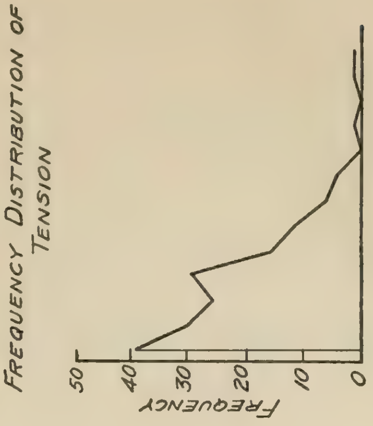

FIGURE 7

tion, special reasons for anticipating heterogeneity here. These reasons consist of peculiarities in the attitudes of the subject toward work which seemed to lead to heterogeneity in the performance. This point will be discussed in more detail later.

To measure the departure of the distributions from normality skewness and kurtosis were tested, the methods used being the same as those used in Section II, p. 22. These results appear in Table IX. The excess of skewness over that of the normal distribution is the difference between the obtained skewness and 0, the excess of kurtosis is the difference between the obtained kurtosis and 3. The reliabilities of these excesses are shown in the third and fourth lines of Table IX.

TABLE IX

Skewness and Kurtosis of the Distributions of Measures of the Work Curves

	Work Rate	*Change of Work Rate*	*Contraction Height*	*Change of Contraction Height*	*Irregularity*	*Tension*
Skewness	.839	–1.441	.234	–.041	1.122	1.300
Kurtosis	3.291	6.880	2.720	3.341	5.830	5.575
$\frac{\text{Excess Skewness}}{\text{S.D.}}$	4.39	7.54	1.23	.21	5.87	6.81
$\frac{\text{Excess Kurtosis}}{\text{S.D.}}$	.76	10.16	.76	.89	7.41	6.74

Table IX bears out the accuracy of the analysis made above on the basis of the appearance of the curves. The distributions of contraction height and change of contraction height do not depart significantly from normal. The other distributions all show reliable skewing and are leptokurtic in shape. The kurtosis of the work rate distribution is not excessive, but work rate does show significant skewness.

It is evident that four of the distributions are not fitted by the hypothesis of normality. This can also be seen in Table XI, where departure from normality has been tested by Chi-Square. The Chi-Square discrepancy between the normal and the obtained frequencies is significant for the four distributions showing reliable non-normality on the basis of the tests of skewness and kurtosis. The question arises of whether the hypothesis that the distributions are heterogeneous gives a better fit to the data. The simplest form of this hypothesis is postulating two normal distributions with the same variance but differing in their means.

As is pointed out by Hotelling (6), if we mingle two distributions with the same standard deviation but with different means, the kurtosis of the mingled distribution will exceed that of the normal distribution by

$$(p - 7p^2 + 12p^3 - 6p^4)\ w^4$$

where p is the proportion of cases drawn from one of the original distributions (for definiteness we may take the one with the smaller mean), and w is the difference between the means divided by the standard deviation of the mingled distribution. Similarly the difference in skewness between the mingled distribution and the normal distribution is

$$(p - 3p^2 + 2p^3)\ w^3$$

For values of p less than .211 or greater than .789 the kurtosis of the mingled distributions will exceed that of the normal distribution (*i.e.*, the distribution will be leptokurtic); for values between .211 and .789 the mingled distribution will be platykurtic. The kurtosis will be maximum for $p = .0917$ (or $= 1 - .0917 = .9083$) and will be minimum for $p = .50$. For $p = .50$ the skewness will be zero, for other values (except of course 0 or 1) there will be some degree of skewness, the maximum skewness being obtained for $p = .211$ or .789, values which gave zero excess of kurtosis.

To estimate the original distributions from which a mingled distribution has been drawn, we may set the expression for the excess of kurtosis due to mingling equal to the obtained excess of skewness and solve the resultant equations. If we designate the obtained excess of kurtosis by K and the obtained excess of skewness by S, the equations are:

$$\begin{aligned} (p - 7p^2 + 12p^3 - 6p^4)\ w^4 &= K \\ (p - 3p^2 + 2p^3)\ w^3 &= S \end{aligned}$$

Eliminating w between the two equations, we obtain an equation of the sixth degree in p. The substitution of y for $p\ (1 - p)$, reduces the sixth degree equation in p to a cubic in y. This cubic has the form $\left(\text{letting L represent } \frac{S^4}{K^3}\right)$

$$y^3 - \frac{y^2}{2} + \frac{(1 + 18L)y}{16 + 216L} - \frac{L}{16 + 216L} = 0$$

Substituting the value of L obtained from the data, in this equation we can find y and from it, p. Substituting in one of the original equations we obtain w. The means and the standard deviation of the two original distributions may then be found from the following equations:

$$m_1 = M - (1 - p)\ w\ S$$
$$m_2 = M + p\ w\ S$$
$$s = S\sqrt{1 - p(1 - p)\ w}$$

Here m_1 and m_2 are the means of the original distributions and s is the standard deviation of the original distributions, while M and S are respectively the mean and standard deviation of the mingled distribution. It should be noted that in finding p from y two values are obtained. If the skewness is positive in sign, p is the larger of the two values (the other value is $1 - p$); if the skewness is negative in sign p is the smaller of the two values. On this basis, p will be the percent of our distribution drawn from a population with mean equal to m_1 and standard deviation, s (these are of course estimates only of the percent, mean, and standard deviation); and $(1 - p)$ will be the percent of our distribution drawn from a population with mean equal to m_2 and standard deviation, s. The parameters of the original distributions, on the hypothesis of heterogeneity outlined here, have been estimated and appear in Table X, along with the means and standard deviations of the obtained distributions. Since contraction height and change of contraction height, appeared to be approximately normal, the hypothesis of heterogeneity was not applied to them.

TABLE X

ESTIMATED PARAMETERS ON THE HYPOTHESIS OF TWO MINGLED NORMAL DISTRIBUTIONS WITH EQUAL VARIANCE AND ON THE HYPOTHESIS OF A SINGLE NORMAL DISTRIBUTION

Single Distribution	*Work Rate*	*Change of Work Rate*	*Irregularity*	*Tension*
M	13.36	– .064	6.38	5.68
S	5.60	.334	2.49	4.38
Mingled Distribution				
m_1	11.39	--1.127	6.04	4.79
m_2	22.14	– .021	13.71	17.18
s	3.24	.241	1.99	3.32
p	.816	.045	.956	.928
w	2.10	3.32	.308	2.83

To test the relative agreement with the two hypotheses, Chi-Square was computed for (1) the frequencies expected on the basis of a single normal distribution; (2) the frequencies expected on the hypothesis of two mingled normal distributions. The results of this test appear in Table XI.

TABLE XI
CHI-SQUARE TEST OF AGREEMENT WITH SINGLE DISTRIBUTION HYPOTHESIS AND WITH MINGLED DISTRIBUTION HYPOTHESIS

	Work Rate	*Change of Work Rate*	*Contraction Height*	*Change of Contraction Height*	*Irregularity*	*Tension*
Single Distribution						
Chi-Square	38.12	77.13	15.68	8.72	29.80	34.04
Degrees of Freedom	10	6	11	9	7	7
Probability	.001	.00001–	.17	.50	.0001	.0001
Mingled Distribution						
Chi-Square	9.96	25.76			5.45	20.86
Degrees of Freedom	8	4			5	4
Probability	.27	.0004			.37	.0004

Comparing the probabilities of getting the obtained results on the basis of the two hypotheses, we see that the results are more in accord with the hypothesis of mingled distributions. In the case of work rate and irregularity, the mingled distribution is in good agreement with the results. The agreement for the change in work rate is poor, but better than the agreement obtained with a single normal distribution. For tension the agreement also is poor but this is anticipated, for the non-normality of the tension distribution seems to be due to curtailing. If we set up the hypothesis that because of the restrictions of the apparatus, all cases below zero became concentrated on the lowest step of our distribution and calculate Chi-Square for this hypothesis, we get a value of 7.18. The number of degrees of freedom is here 6 and so the probability of the result is .31, indicating a good agreement with the hypothesis.

If now we consider the results obtained, we find that they are not mere arbitrary statistical constructs. There seems to be a definite logical explanation underlying the peculiarities of the obtained distributions. The hypothesis of mingled distributions fits well the distribution of work rate and the distribution of irregularity. In Section II, p. 21, we mentioned the tendency for some individuals to work under "high pressure." We suggested there that the obtained correlations were explicable on the basis of a drive to maintain speed, which resulted in lowered height of contraction and decreased regularity. If we refer to Table X, we see that 81.6 percent of the group can be thought of as belonging to a distribution with an average rate of work at 11.39 pulls per 10 second interval, while 18.4 percent of the group would fall into a distribution with an average rate of work at 22.14 pulls per 10 second interval, a speed almost twice that of the rest of the group. This is quite consonant with the explanation that most of the group are content with exerting what seems to them a fair amount of speed, while a small number of individuals strive for a high speed, possibly sacrificing contraction height to achieve that high speed. The individual's work rate will be determined by many other factors besides attitude toward speed. The net effect of these other causes would be to produce a distribution of speeds varying continuously from very slow to very fast. The effect of work attitude is to produce a distribution of speed at two points, one point representing work rate in cases where emphasis on speed is present, the other representing work rate in cases where it is absent. The net results give an appearance of continuity to really discontinuous variation. The discontinuity of the variation becomes apparent when we have only one factor operative.

In our task, attitude toward speed cannot be represented as a continuously distributed variate, with some individuals emphasizing speed more and some less, but all putting some emphasis on this factor. We have rather some individuals emphasizing speed and some not emphasizing it, resulting in a separation into two groups. If attitude were the only factor determining speed, we might expect to have a complete dichotomy. However, here, as elsewhere in psychology, the causal factors underlying the things we measure are complex and not simple, so that unitary dichotomy of causes is overlaid by "continuous" variation and complicated procedures become necessary for causal analysis.

The distribution of irregularity seems to be non-normal because of the association of irregularity with high speed. The fast workers seem to be also the irregular workers. The speed is probably the primary variable with irregularity as a secondary effect. It is also likely that some part of the tension non-normality is due to its association with speed (the fast workers do not have time enough to go back to the baseline after each pull), although most of the distortion of the tension distribution is attributable to curtailing.

Contraction height and change in contraction height seem to be most closely related to capacity for work and to muscular strength (although the faster workers do seem to give lower contraction heights, and they might be able to better their contraction heights at lower speeds). This capacity seems to be normally distributed and these measures are, similarly, normally distributed. Since there is no reason for supposing that contraction height is merely the negative of speed, the normality of the contraction height distribution is not extraordinary.

In the case of change of work rate we have a rather different situation. This distribution corresponds neither to a single normal distribution nor to a mingling of two normal distributions with equal variance. If we examine the distribution curve (Figure 2) of change of work rate, we find it markedly leptokurtic (there is a marked piling up of cases around the mean). The mean incidentally is not very far from zero. The mean is – .064 and is only 2.43 times its standard error. This is not a significant difference from zero, and is to be contrasted with the average value of – 1.11 for change of contraction height, which is over 21 times its standard error. There seems to be a tendency for the group as a whole to maintain its original speed, so that decline in speed is slight, while decline in height of contraction is large. In a few cases, however, this tendency toward constant speed is markedly abandoned. In other cases there seems to be an actual increase in speed. If we suppose that constant speed is the characteristic reaction of the majority of the subjects, there seem to be two possible explanations of the results: (1) the subjects are actually drawn from two different normal populations, one population composed of the bulk of the subjects and concentrated around zero or some small negative value with a comparatively small standard deviation, and the other population composed of a small percentage of the subjects with a mean possibly the same as the other group but with a much larger standard deviation; (2) the subjects are actually drawn from three different normal populations having the same variability, but the mean of one population falls above zero, of another at zero, and of the third far below zero.

While the second hypothesis is a possible one, its psychological significance is difficult to discover. The first hypothesis has, however, a relation to a number of interesting theoretical implications. There seem to be more grounds for postulating two modes of response, a constant speed and a variable speed response, than there are for postulating three modes (increasing, constant, and decreasing).

Firstly, with regard to the tendency towards constancy in speed which distinguishes the majority of subjects, there is the work of Morgan (16). Morgan found that in lifting weights subjects were much better able to keep their speed constant than to keep their force constant. We may assume that the directions disposed the subjects to keep both speed and height of contraction constant. Height of contraction, however, may be much more difficult to estimate than is speed since it involves (in the spring ergograph) force of movement as well as distance moved. The constancy of speed in our results may be an indication of the fact that this factor is more easily controlled than are the other characteristics of the curve. The second group of subjects may consist of those who were unable to control speed either because of some original inaccuracy or because of their greater emphasis on other features of the task.

Secondly, we have a possible relation to the results of those experimenters who have attempted to link the curve with Kretschmer's theory of body types. Enke (2) found that, while all groups tended to maintain a constant tempo (which agrees with our own findings), the emphasis on regularity is much greater in leptosomes than it is in pyknics. In the age range of our subjects the percentage of pyknics is not large, and it is possible that the secondary population represented in our distribution of change of work rate is composed of pyknics, while the larger part of our sample would consist of leptosomes, athletics, and mixed types.

We attempted to determine whether any evidence could be discovered which would indicate a difference in change of work rate between pyknic subjects and leptosome subjects. The ratio of body weight to height, while it is not the best possible measure, gives an indication of physique which will serve for a rough analysis. We first attempted to correlate change of work rate with the ratio weight over height. The correlation obtained was $-.116$, and for this correlation z is only 1.44 times its standard error.

In work with types, however, correlation may prove misleading since the exponents of type theory do not claim a single linear relationship existing between traits, but rather differences in personality corresponding to differences in physique. These differences can be analyzed in terms of group differences but not in terms of correlation. We therefore next selected the ten members of the group having the highest weight-height ratio (roughly a ''pyknic'' group) and the ten individuals having the lowest weight-height ratio (roughly ''leptosome'').

Table XII gives the average change of work rate for the two

TABLE XII
DIFFERENCES BETWEEN THE 10 SUBJECTS WITH HIGHEST WEIGHT-HEIGHT RATIO AND THE 10 SUBJECTS WITH LOWEST WEIGHT-HEIGHT RATIO. MEASURES OF THE FIRST CURVE OF EACH SUBJECT

	Mean		*S.D.*		*t**	*z***
	Highest	*Lowest*	*Highest*	*Lowest*		
Work Rate	15.80	11.26	6.17	4.09	1.94	.4106
Change of Work Rate	–.124	–.017	.497	.123	.87	1.3982
Contraction Height	24.58	27.58	14.07	12.70	.50	.1025
Change of Contraction Height	–.72	–1.25	.72	.68	1.69	.0517
Irregularity	6.19	6.07	3.36	2.25	.09	.4209
Tension	6.17	5.13	4.25	3.30	.61	.2520

* t = 2.878 corresponds to a probability of .01.
** z = ±.8386 corresponds to a probability of .01.

groups as well as the averages of the other measures of the groups. Table XII also gives the standard deviations of the measures. The column headed t represents the ratio of the difference between the means of the two groups over the standard error of the difference. For a sample of this size (n = 18), t equal to 2.878 corresponds to a probability of .01. None of the values of t in the table is as large as 2.878 so that none can be considered significant.

The means of the two groups are not really adequate measures of the type of difference in which we are interested, for in the mean large increases of work rate will balance large decreases of work rate, so that, even though pyknics may show large *absolute change* of rate, their mean increase or decrease of rate may not differ at all from that of a leptosome group with an actually smaller absolute change of work rate. If we take the mean absolute change of work rate (change without regard to direction), the difference between the "leptosome" and "pyknic" groups is increased, the respective means being .101 (leptosomes) and .355 (pyknics). It is not possible to make an accurate test of the significance of the difference here, since, if the population of changes of work rate is normal, the population of absolute changes in rate cannot be normal, so that t will be misleading for one or both of these variates. In addition the non-normality of the general distribution of change of work rate may affect the validity of t as a measure of significance.* If the population of absolute change of rate were normal, the value of t found (2.24) would not be significant. The adequacy of the test of significance is very questionable in our sample.

We suggested above, however, that the secondary group indicated by the non-normality of the distribution of change of rate might differ in variance as well as in mean. If the pyknic group is the secondary group of our distribution, some test of the variance of the two weight-height extremes should be made. If we estimate the standard deviations of the "pyknic" and "leptosome" groups, we find .497 for the "pyknics" and .123 for the "leptosomes." This difference may be tested by getting z (equal to the difference between the natural logarithms of the standard deviations) and referring to the distribution of z (Table VI of Fisher, 4) for the probability that a difference of this size would arise by random sampling. In this case z is 1.3982 and the probability is less than .01. The test case z is 1.3982 and the probability is less than .01. The test of differences in variance is much more valid statistically than the t test used above, for here the hypothesis tested is that the pyknic and leptosome groups are normally distributed around the same or different means with the same variance There seems to be a reliable difference in variability between the group with high weight-height ratios and the group with low weight-height ratios. This difference is roughly in accord with that expected on the basis of Kretschmer's theories. Since the group was selected on the basis of weight-height alone, it is a random sample except with regard to that variable (although there seem to be certain other characteristics distinguishing these subjects, *e.g.*, the "leptosome" group is about one year younger on the average, and they are somewhat lighter than the "pyknic" group, so that the difference in weight-height ratio is due more to weight than to height.)

Although there seemed to be no peculiarity of sampling in the selection of the groups with high and low weight-height ratios, it was felt that a further check would be desirable. Two new groups of pyknics and leptosomes were selected, this time on the basis of subjective impression (the method more commonly used in classifying Kretschmer's types). Here the shape of the face, the distribution of fat, and other factors were considered. In each case, only those cases felt to be definitely "pyknic" or definitely "leptosome" were included. In this basis 8 "pyknics" and 15 "leptosomes" were selected from the group of 164 subjects. Two of this group of "pyknics" (*i.e.*, 25% of the group) were among the group having the highest weight-height ratios, while four of the "leptosomes" (27%) were among the 10 individuals having the lowest weight-height ratios. The agreement of selection on the basis of weight-height ratio with the subjective selection is not large, but four factors contribute to the difference: (1) inaccuracy of the weight-height ratio as a measure of pyknic tendency; (2) inexperience of the classifier in judging Kretschmer's types; (3) a greater emphasis on height as against weight in selection of the leptosomes (note above the greater influence of weight differences in the weight-height ratio); (4) the rejection of all cases in which the classifier was not sure of the diagnosis. A further inaccuracy in the classification is the fact that the judgments were made on the basis of the subject's appearance fully clothed, while in most studies of body type, diagnoses have been made with the subject completely stripped.

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* The non-normality of the distributions makes it also necessary to take the reported reliabilities of the correlations of Table I, p. 00, with extreme caution.

Granting all these inaccuracies, the results of this new classification confirm those of the classification on the basis of weight-height ratio alone (see Table XIII). Here again we find no significant differences in mean change of work rate, but a significant difference in variance does appear. As a matter of fact the mean change of

TABLE XIII

DIFFERENCES BETWEEN 15 "LEPTOSOME" SUBJECTS AND 8 "PYKNIC" SUBJECTS. MEASURES OF THE FIRST CURVE OF EACH SUBJECT

	Mean		*S.D.*			
	Pyknic	*Leptosome*	*Pyknic*	*Leptosome*	*t**	*z***
Work Rate	12.80	13.82	4.79	5.411	.45	.1215
Change of Work Rate	–.004	–.054	.410	.162	.43	–.9293
Contraction Height	24.29	27.77	10.55	8.79	.84	–.1818
Change of Contraction Height	–.887	–1.24	.531	.772	1.14	.3747
Irregularity	6.53	6.53	2.01	3.50	.0	.5520
Tension	5.16	5.49	3.95	4.54	.17	.1378

* $t = 2.831$ corresponds to a probability of .01.
** $z = .9105$ corresponds to a probability of .01.
$z = -.7264$ corresponds to a probability of .01.

rate is greater for the "leptosomes" than it is for the "pyknics." If, however, we take the absolute mean change, the pyknic group does show a larger mean (.324 as against .127) and the difference is significant ($t = 2.90$). The standard deviations of the two groups are .162 (leptosomes) and .410 (pyknics). The value of z for these standard deviations is $-.9293$ and this is a significant difference.

Because of the small size of the sample additional checks were felt to be necessary. We therefore repeated for their second curves the comparison of the 10 subjects with high weight-height ratio and the 10 with low weight-height ratio. This was also done for the subjectively selected "pyknic" and "leptosome" groups. The results appear in tables XIV and XV. None of the differences of means is significant. The group with low weight-height ratio differs significantly in variance from the group with high weight-height ratio. When we turn to the "leptosome" and "pyknic" groups which were subjectively selected, we find that the difference in variance has been reversed, although the reversal does not yield a significant difference.

It is interesting that the only significant differences of the groups appear in accordance with the hypothesis we advanced to account for the form of the distribution of changes of work rate. The fact that the differences in variability are reversed in one instance makes it difficult to conclude that the differences support Kretsch-

TABLE XIV

Differences Between the 10 Subjects with Highest Weight-Height Ratio and the 10 Subjects with Lowest Weight-Height Ratio. Measures of the Second Curve of Each Subject

	Mean		*S.D.*		*t**	*z***
	Highest	*Lowest*	*Highest*	*Lowest*		
Work Rate	18.28	13.75	8.05	5.76	1.45	.3360
Change of Work Rate	–.141	.051	.274	.112	2.05	.8967
Contraction Height	26.36	26.99	13.60	13.03	.11	.0428
Change of Contraction Height	–.779	–.945	.769	.609	.54	.2289
Irregularity	7.27	7.43	3.72	2.53	.11	.3875
Tension	6.73	5.21	4.64	5.50	.79	–.3402

* $t = 2.878$ corresponds to a probability of .01.
** $z = \pm .8386$ corresponds to a probability of .01.

mer's theories. It is possible that the association of weight-height ratio and work rate may be explained in terms of the relative muscular strength of the groups with low and high weight-height ratios. The 10 subjects with the lowest weight-height ratio (leptosomes) give a higher mean contraction height than the subjects with the highest weight-height ratios, possibly indicating greater strength (the difference is, however, not significant). None of the other differences is high, and there seems to be no other confirmation of Kretschmer's theory in the data. The most likely explanation of the difference in variance of change of work rate is a purely statistical one. Since the distribution of the entire group is markedly leptokurtic (see Figure 3), the probability of obtaining a given difference in variability (or a given value of z) is greater than that indicated in Fisher's Table VI (which is based on an assumed normality of the population of individual observations).

TABLE XV

DIFFERENCES BETWEEN 15 "LEPTOSOME" SUBJECTS AND 8 "PYKNIC" SUBJECTS. MEASURES OF THE SECOND CURVE OF EACH SUBJECT

	Mean		*S.D.*		*t**	*z***
	Pyknic	*Leptosome*	*Pyknic*	*Leptosome*		
Work Rate	14.39	15.83	5.71	8.23	.44	.3653
Change of Work Rate	–.034	–.060	.236	.343	.18	.3855
Contraction Height	24.93	30.77	10.76	8.28	1.45	–.2625
Change of Contraction Height	–1.275	–.800	.440	.630	1.89	.3584
Irregularity	7.07	7.35	2.96	3.42	.20	.1651
Tension	6.31	7.37	5.22	6.21	.41	.1341

* $t = 2.831$ corresponds to a probability of .01.
** $z = .9105$ corresponds to a probability of .01.
$z = -.7264$ corresponds to a probability of .01.

Although the results on change of work rate may be partly in agreement with Enke's findings, no conclusion can be drawn, first because of the presence of a difference running contrary to the positive instances, and second because of the statistical difficulties mentioned above. These statistical reservations emphasize once more the importance of normality in tests of significance when samples used are small. The obtained "significant" differences in variability point to the necessity for a check on normality, before we proceed to place reliance in a given statistical finding. We also wish to suggest the possible rôle of factors of strength, age, racial and cultural background, etc., in the differences supposedly associated with body type. It is altogether likely that the association with body type may be secondary to an association with these more fundamental factors.

SUMMARY

In analyzing the shape of distributions of measures of the work curve, reliable departures from normality were found for work rate, change of work rate, irregularity of the curve, and tension (failure to relax). The tension non-normality was traceable to the nature of the apparatus. The non-normality of the distribution of work rate and of irregularity of the work curve were shown to accord with the hypothesis suggested in Section II that certain individuals place great emphasis on speed in the ergographic performance, and consequently give results which distinguish them from the rest of the group. The frequency curve for change of work rate suggested the presence of two groups differing in variability and possibly also in mean value. Some evidence (not wholly consistent) was found, linking differences in physique (Kretschmer's types) to the two groups suggested as populations from which the change of work rate sample was drawn. The other findings showed no particular relation to Kretschmer's theories.

SECTION V

PSYCHOMOTOR CONSISTENCY AND THE PROBLEM OF TYPES

One of the great defects of much of the work which has been done in the field of personality research is the inadequate treatment of sampling errors. For example, the work of Allport and Vernon (1), excellent as it is in its conceptual and methodological aspects, is quantitatively dubious due to the neglect of the effects of sampling. The principal quantitative result offered by Allport and Vernon is their discovery of three group factors in psychomotor behavior (their ''areal,'' ''centrifugal,'' and ''emphasis'' factors). In locating these factors a combination of intuitive (or logical) and statistical procedures was used. The sampling distribution of the resulting statistics is undoubtedly very complex. It is likely that the method used by Allport and Vernon makes it practically impossible to determine the probability that the results obtained might arise on a basis of random sampling.

Probably the most valuable contribution of Allport and Vernon to the field of personality study is their insistence on the inadequacy of correlational methods in the detection of subtle and complicated kinds of relationships. As was stated in the Introduction, we do not believe that the concept of ''congruence'' really solves the problems raised by the inadequacies of the correlational technique. It has, rather, the general effect of removing the investigation from a region of over-simple but definite relation to a field of subjective vagueness, which may have greater scope than the correlational field but which makes research depend on individual opinion as to what is ''harmonious'' or ''integrated.'' If we turn away, for a time, from the subjective approach, we find that there are additions to the correlational technique which may aid in widening its scope without reducing its definiteness.

Correlation is merely a special case of the broader concept of dependence. The correlation coefficient ''measures'' the ''amount'' of linear dependence of two variates. It fails, however, to measure adequately the great group of cases which show dependence of variates of a sort more complicated than simple linear regression. In some cases, the correlation ratio may give us an estimate of such dependence. A better estimate can be obtained by fitting regression curves and testing the coefficients for significance. There remains, however, a small but important group of cases in which the relationship of the two variates is not expressible in simple mathematical terms. The relation here may be very complex and the assumption of any of the more usual forms of dependence will not give accurate results.

In cases where dependence is felt to exist but correlational techniques prove inadequate, the method of contingency may be useful. The method can be applied to both continuous and discontinuous variates, it makes no assumption about linearity of regression or normality of distribution, and the units of measurement do not have to be equal. The method measures simply the tendency of the distribution of one variate to differ for different values of another variate. One defect of the method is the number of degrees of freedom used up in estimating expected values. Another difficulty is the necessity of avoiding small frequencies in a single cell. At the same time the use of fine grouping (for continuous distributions) is desirable. These last two requirements make the use of large samples imperative, if any great accuracy is to be attained.

In our case several measures of the work curve showed no significant correlation with each other. These non-significant correlations are given in Table XVI. It was thought that dependence of the variates might nevertheless exist. The scatter diagram of the correlation coefficient is easily converted into a contingency chart by combining cells in which the frequencies are small. The frequency expected in each cell on the hypothesis of independence

TABLE XVI

Test of Correlation Tables for Non-Linear Dependence

Variates	*Correlation*	*Chi-Square*	*Degrees of Freedom*	*Probability*
Work Rate—Change of Work Rate	–.069	13.6	20	.85
Work Rate—Change of Contraction Height	–.115	40.7	25	.03
Change of Work Rate—Contraction Height	–.032	14.8	24	.92
Change of Work Rate—Change of Contraction Height	–.104	23.5	15	.07
Change of Work Rate—Irregularity	–.114	15.7	16	.45
Change of Work Rate—Tension	–.160	17.1	13	.20
Change of Contraction Height—Irregularity	–.031	18.6	15	.23
Change of Contraction Height—Tension	.101	24.0	20	.24

is the product of the marginal totals over the number of cases. The departure from the hypothesis can be tested by the Chi-Square distribution. The Chi-Square values found for the various pairs of measure used are given in Table XVI, which also shows the number of degrees of freedom and the corresponding probabilities. It is evident that this test agrees with the correlation in showing no dependence of the variables involved. There may be a dependence but it is of too small an order to be detected by available methods.

A further analysis of dependence might be made by taking three variates at a time and constructing a three-dimensional contingency table. In cases in which a test of the total interrelationship of three or more variates is desired, this might be a very valuable procedure. In the present case it was not felt that anything would be shown by such a procedure, so it was not undertaken. The number of variates which can be handled by this method is limited only by the number of available cases and by the experimenter's patience.

In certain cases it might be desirable to know whether there was any dependence in addition to that indicated by an obtained correlation coefficient. (This is really the problem in Table XVI but the obtained correlation here is zero). Pearson (19) gives a table of the bivariate normal distribution from which it is possible to estimate frequencies to be expected in a given region of the distribution when a certain correlation is supposed to exist. With these frequencies a Chi-Square test can be made to determine whether relation in some regions may not be greater or less than that indicated by the correlation. To generalize this procedure to three variates a somewhat different procedure may be necessary. It has been suggested that the best method here might be to apply a transformation to the original variates, such as that given by the Principal Component Method of Hotelling (7), and to test the transformed variates for dependence. This can be done by the contingency method. For greater sensitiveness, the expected values of each variate can be substituted for the marginal totals, leaving many more degrees of freedom available in entering the Chi-Square table. This last procedure was not followed in the present study because of the obvious non-normality of the univariate distributions (see Section IV).

In the present study the fact that the distributions of the work curve measures were not normal was both a disadvantage and an advantage. The non-normality of the distributions made impossible the rigorous application of many of our usual statistical techniques. On the other hand, the fact of non-normality called attention to phenomena which might otherwise not have been detected. Probably the most interesting of these phenomena is the appearance of discontinuity in supposedly continuous variation. In Section IV we suggested that the non-normality of the work rate distribution might be attributed to a discontinuity in attitudes toward speed. This assumption may seem unwarranted, but it derives directly from the reasoning underlying the hypothesis of the mingling of distributions, and the results obtained seem to fit the hypothesis rather well. Most of the analysis of Section IV is the same as that applied by Hotelling (6) in a study of duration of pregnancy. This study also brings out the causal discontinuities in apparently continuous frequency distributions. Hotelling found that his distribution of duration of pregnancy indicated (on the basis of an analysis for mingling of two normal distributions) that in a small proportion of the cases an error of about a month had been made in the estimated menstruation-to-birth duration. This error is attributable to a ''false menstruation'' occurring after the beginning of pregnancy. All of the cases in Hotelling's sample were subjected to causal factors which tended to produce a continuous, normal distribution of pregnancy duration. In addition to these causes of continuous variation there was the error in estimating the beginning of pregnancy. This error does not produce continuous variation of duration of pregnancy (occurrence of ''false menstruation'' would cause an error of one month or nothing). If variation from other causes could have been eliminated, the distribution would fall into two distinct and separate classes with the estimated duration of pregnancy of all members of one class differing by one month from the durations of members of the other class.

The phenomena of classification as against continuous variation is one of the concepts which is associated with the theory of ''types.'' The term ''type'' has been used in numerous connections, many of which do not really require the term (as is pointed out by Klüver, 11, in a critical discussion of type theory). Klüver suggests that a theory of ''types'' is to be distinguished by its insistence on modes of organization as a basic feature of the organism. It is not desired here to enter into a fruitless controversy over what is the essential feature of type theories. The writer wishes however to call attention to the rôle of discontinuity and of distribution into classes or modes in types.

One of the persistently recurring concepts of type theories is the idea that some individuals resemble each other very much and differ from other groups of individuals. Thus persons are thought of as falling into distinct classes, whose members all have some common similarity or similarities. In Hotelling's distributions of pregnancy durations there are two ''types'' of duration which are distinguished by the presence or absence of the ''false menstruation.'' In our own data on work rates we seem to be dealing with two ''types'' of workers, the two ''types'' being distinguished by their respective attitudes toward speed. In the distribution of changes of work rate, there again seem to be two types, one distinguished by a uniform work rate and the other by a changing work rate.

This distribution into classes is not an exceptional phenomena. A continuous distribution by no means implies continuity of causation. The theory of the normal distribution recognizes the fact that identical values of a variate may derive from quite diverse causal patterns. The tendency in psychology, however, has been to regard similar scores as uniform in import although they are probably qualitatively and causally distinct. Our analysis emphasizes the fact that two individuals with the same work rate may belong to markedly different work rate distributions.

The analysis made of the ''pyknic'' and ''leptosome'' groups seems to be particularly relevant to the theory of types. There seems to be some relation between weight-height ratio and work rate changes. (The relation is not clear-cut, but it will suffice for purposes of illustration.) Now we may conclude that ''pyknic'' and ''leptosome'' groups differ in their work characteristics. This is, however, only a partial picture of the results. If we analyze further we may be able to discover why these groups differ. Some of the causes underlying the results on the weight-height ratio have been suggested in Section IV. Ultimately the differences between ''pyknic'' and ''leptosome'' groups must be reducible to the presence or absence of certain causal factors which determine both body constitution and the various traits which have been associated with body constitution.

We have suggested that our results are indicative of ''types'' distinguished by their attitudes toward speed. It was further suggested that these types represent an actual discontinuity in work rates plus a ''continuity'' of individual variation. It should be emphasized that this discontinuity in the measurable variables does not imply any discontinuity in the psychological law underlying the variables. Type theories frequently seem to suggest that a set of measures may show one "dynamic" relationship in one part of their range and other relationships in other parts. Thus a type theory might suggest that the eidetic imagery of people with a thyroxin excess is not merely different from that of people with deficient parathyroid secretion, but that the relation of thyroid secretion, parathyroid secretion, and eidetic imagery is different for the two "types." Superficially we may have "types," but a further analysis will most likely disclose a single relation which will cover both the "types" with thyroid excess and the type with parathyroid deficiency, as well as any other "type" of relation of the three variables. In most cases the "type" is due to the presence of some other variable or variables, which complicates the relation (in the same way as, in Newtonian mechanics, the presence of other factors may complicate an essentially simple relation between the acceleration of a certain body and a given applied force). In other cases the "type" is due to a failure to see a more inclusive relationship in the dependence of variables (*e.g.*, in mathematics the parabola and the ellipse are superficially distinct "types" of curve, but they both come under the more inclusive relationship of conic sections and both can be specified by a single "type" of equation).

In type theories more than one "mode of organization" may arise from failure to consider primary causal sources. Thus we might seem to get a "type" in change of work-rate with differences in weight-height ratio as part of the type make-up. Closer analysis may, however, reveal that the association between weight-height difference and work rate constancy is a matter of the causal dependence of work rate on strength, and the association of strength with weight-height. Another possibility might be the existence of certain cultural or economic differences having causal relation to weight-height because of associated nutritional factors and causal relation to work rate because of associated peculiarities of education. Thus we might get a "work-type" difference between Indian and White children and also an associated difference in skin color. We might find differences of skin color of no importance in determining work rate within a given group, but very significant in the determination of differences between the groups. We might then conclude that there are two "types" in the distribution of these two variates, a dark-skinned, low work-rate group (Klineberg, 10, found certain groups of Indian children whose speed was low because of tribal attitudes toward speed) and a light-skinned, high work-rate group. A ''dynamic relation'' of skin color and work rate would thus supposedly be uncovered, and we could probably uncover other features which would go along with this ''dynamic relation.'' The actual relation of skin color to work rate would, however, have nothing to do with this ''type'' theory.

It is likely that types can ultimately be reduced to terms involving relations which hold generally, instead of only for a certain region of variation of phenomena. On the other hand the type method may provide a necessary preliminary step in the development of the general relation. In any case, it may hold the investigator closer to empirical fact and prevent premature generalization on an over-simplified level. The main value of type theories may be in restraining naive unification of discordant observations. The insistence of the typologist on the real discontinuity between various ''modes of organization,'' ''Gestalten,'' ''response patterns,'' etc., may be a valuable counterbalance to the too-facile generalizations of those in whom the drive for simplicity and scientific unity overpowers the caution which should be engendered by the complexity and multiplicity of the material dealt with. But type theory must remain to a large group of psychologists merely a temporary expedient in the ultimate development of general laws of behavior. The insistence on differences in modes of integration is not a satisfactory answer to the question of the organization of personality. The existence of these differences must (if only because of our faith in the uniformity of natural law) imply the existence of a difference in causation. The writer believes that the causal factors underlying types may be expressed as simple and unitary factors which act on each other in a purely ''atomistic'' fashion. Of course the ''dynamic'' picture may get closer to the ''real nature'' of the relations, but the ''real nature'' of objects is not an essential concern in the construction of a workable psychological system. The development of a self-consistent system, which allows abstract manipulation of quantities (or qualities) and which permits accurate prediction of future events, seems to the writer more desirable than the solution of the philosophical problem of the ''nature'' of psychological relationships. The writer has no antipathy towards this problem, and we may even find that ''dynamic'' relationships cannot be handled in terms of analytic abstractions.

For the present, however, the analytic goal is probably most desirable. Whether we adhere to an analytic or a synthetic approach, it seems particularly necessary to guard against the too ready acceptance of vague theories which happen to label themselves "dynamic." There may still be elements of value in the "narrow," "static," formulations of less "recent" psychologies.

SUMMARY

1. Two records of work on the spring ergograph were obtained from each of 164 subjects. For each curve measures were obtained of work rate, change in work rate, contraction height, change in contraction height, irregularity of the curve, and failure to relax after pulls on the spring. For a sample of 50 subjects, third order regression equations were fitted to the contraction heights.

2. The correlations between the two measures obtained from each subject range from .555 to .790, indicating a fair degree of consistency in the individual's work curves. The "shape" of the curve does not seem to be consistent for a given individual (taking the coefficients of the third order regression equation as measures of this feature). The lack of consistency in the shape of the curve contrasts with the results for the weight ergograph reported by other investigators.

3. In general the shape of the curve of work on the spring ergograph (average curve for 50 subjects) is concave, showing a tendency for the subject to reach and maintain a level of constant output. In the weight ergograph a decrease to a point of complete decrement usually occurs. Our curves resemble those for mental work rather than the weight ergograph curves.

4. The intercorrelations of the measures of the work curve and analysis of the shape of their distributions indicate that individuals differ in their modes of response to the task. Some subjects spontaneously assume an attitude emphasizing the maintenance of a high rate of speed. This attitude seems to produce a heterogeneity in the frequency distribution of work rates.

5. Analysis of the frequency distributions of the measures of the work curve suggests the possibility of tracing the effect of discontinuous causal factors in a continuous distribution of measures.

6. In comparing groups of approximately "pyknic" constitution with groups of approximately "leptosome" constitution, differences in variance of work rate changes were found. The differences in variance are significant in three cases out of four (the fourth case reverses the difference, however). It is probably possible to account for the differences in terms of random sampling, if we consider the fact that the population of changes of work rate is leptokurtic in form.

7. Contingency methods were used in an attempt to test for dependences of the measures additional to those indicated by correlational techniques. No such dependences were found in our data, but the method, and methods similar to it, may have a general utility in the analysis of non-linear correspondence of variates.

BIBLIOGRAPHY

1. Allport, G. W., and Vernon, P. E. Studies in expressive movement. N. Y., Macmillan, 1933, 269 p.
2. Enke, W. Die Psychomotorik der Konstitutionstypen. Z. ang. Psychol., 1930, 36, 237–287.
3. Fisher, R. A. Moments and product moments of sampling distributions. Proc. Lond. Math. Soc. (Series 2), 1929, 30, 199–238.
4. Fisher, R. A. Statistical methods for research workers. Lond., Oliver and Boyd, 1934, 5th ed., 319 p.
5. Garth, T. R., Ikeda, K., and Gardner, D. A. Japanese work curves. J. Appl. Psychol., 1933, 17, 331–336.
6. Hotelling, H., and Hotelling, F. A new analysis of duration of pregnancy data. Amer. J. Obstet. Gynecol., 1932, 23, 643–657.
7. Hotelling, H. Analysis of a complex of statistical variables into principal components. J. Educ. Psychol., 1933, 24, 417–441; 498–520.
8. Ioteyko, J. Les lois de l'ergographie. Étude physiologique et mathématique. Acad. Roy. Belgique Bull., 1904, No. 5, 172 p.
9. Ioteyko, J. La fatigue. Paris, Flammarion, 1920, 331 p.
10. Klineberg, O. An experimental study of speed and other factors in ''racial'' differences. Arch. Psychol., 1928, No. 93, 111 p.
11. Klüver, H. Do personality types exist? Amer. J. Psychiat., 1931, 10, 781–789.
12. Korngold, S. Contribution a l'étude de la constance des sujets dans les efforts moteurs. Le Travail Humain, 1933, 1, 186–191.
13. Kretschmer, E. Physique and character. N. Y., Harcourt B., 1925, 266 p.
14. Lehmann, H. Messung des Kraftimpulses. Indus. Psychotechn., 1928, 5, 264–272.
15. Maity, H. P., and Sinha, C. P. N. Studies in muscular work by the ergographic method. Indian J. Psychol., 1926, 1, 48–52.
16. Morgan, J. J. B. The speed and accuracy of motor adjustments. J. Exper. Psychol., 1917, 2, 225–247.
17. Mosso, A. Fatigue. N. Y., Putnam, 1915, 334 p.
18. Neifeld, M. R., and Poffenberger, A. T. A mathematical analysis of work curves. J. Gen. Psychol., 1928, 1, 448–458.
19. Pearson, K. Tables for statisticians and biometricians, Part II. Lond. Biometric Lab., Univ. College, 1931, 262 p.
20. Rietz, H. L. Handbook of Mathematical Statistics. N. Y., Houghton M., 1924, 221 p.
21. Robinson, E. S. Work of the integrated organism. Handbook Gen. Exper. Psychol., C. Murchison ed., Worcester, Mass., Clark Univ. Press, 1934, 571–650.
22. Skawran, P. Die Typologie der Ergogramme und ihre Beziehung zu Energietypen und zu den Körperbautypen Kretschmers. Psychotechn. Z., 6, 1931, 10–19; 49–63.
23. Wojciechowski, J. Contribution a l'étude du test avec le dynamographe de Ch. Henry. VIa Conferència Internacional de Psicotecnica, 26–30, April, 1930, Barcelona. Anals D'Orientacio Professional, 351–356.
24. Woodworth, R. S. Le mouvement. Paris, Doin, 1903, 421 p.
25. Yochelson, S. Effects of rest-pauses on work decrement. Yale Univ. unpublished Ph.D. dissertation, May 1, 1930, 102 p.

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Journal of Abnormal and Social Psychology—Eno Hall, Princeton, N. J.; American Psychological Association.
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Psychological Clinic—Philadelphia, Pa.; Psychological Clinic Press.
Subscription $3.00. 288 pages. Edited by Lightner Witmer. Without fixed dates. (Quarterly). Orthogenics, psychology, hygiene. Founded 1907.

Journal of Educational Psychology—Baltimore; Warwick & York.
Subscription $6.00. 720 pages. Monthly except June to August. Edited by J. W. Dunlap, P. M. Symonds and H. E. Jones. Founded 1910.

Psychoanalytic Review—Washington, D. C.; 3617 10th St., N. W.
Subscription $6.00. 500 pages annually. Edited by W. A. White and S. E. Jeliffe. Quarterly. Psychoanalysis. Founded 1913.

Journal of Experimental Psychology—Princeton, N. J.
Psychological Review Company. Subscription $7.00. 900 pages annually. Experimental. Bimonthly. Edited by S. W. Fernberger. Founded 1916.

Journal of Applied Psychology—Indianapolis; C. E. Pauley & Co.
Subscription $5.50. 600 pages annually. Edited by James P. Porter, Ohio University, Athens, Ohio. Bi-monthly. Founded 1917.

Journal of Comparative Psychology—Baltimore, Md.; Williams & Wilkins Company.
Subscription $5.00 per volume of 450 pages. Ed. by Knight Dunlap and Robert M. Yerkes. Two volumes a year. Founded 1921.

Comparative Psychology Monographs—Baltimore, Md.; The Johns Hopkins Press.
Subscription $5.00. 400 pages per volume. Knight Dunlap, Managing Editor. Published without fixed dates, each number a single research. Founded 1922.

Genetic Psychology Monographs—Worcester, Mass.; Clark University Press.
Subscription $7.00 per vol. Bimonthly, one volume per year. Edited by Carl Murchison. Each number one complete research. Child behavior, animal behavior, and comparative psychology. Founded 1925.

Psychological Abstracts—Eno Hall, Princeton, N. J.; American Psychological Association.
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Journal of General Psychology—Worcester, Mass.; Clark University Press.
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Journal of Social Psychology—Worcester, Mass.; Clark University Press.
Subscription $7.00. 500 pages annually. Edited by John Dewey and Carl Murchison. Quarterly. Political, racial, and differential psychology. Founded 1929.